TRANSACTIONS

OF THE

AMERICAN SOCIETY

OF

MECHANICAL ENGINEERS.

VOL. II.

1881.

NEW YORK CITY:

RE-PUBLISHED BY THE SOCIETY,

AT THE OFFICE OF THE SECRETARY,

12 WEST THIRTY-FIRST STREET.

SECOND EDITION.

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Astor Place, New York

OFFICERS

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS

1882–1884.

PRESIDENT:

ROBERT H. THURSTON;
Stevens Institute of Technology, Hoboken, N. J.

VICE-PRESIDENTS:

Wm. H. Shock, U. S. N.,	Washington, D. C., . . .	Term expires Nov. 2, 1882.
Theo. N. Ely, ...	Altoona, Pa., ...	" " " "
Washington Jones, . .	Philadelphia, Pa., . . .	" " " "
Wm. P. Trowbridge, .	New York City, ...	" " Nov. 1, 1883.
E. D. Leavitt, Jr., . .	Cambridgeport, Mass., .	" " " "
Chas. E. Emery, . . .	New York City, ...	" " " "

MANAGERS:

Wm. B. Cogswell, . .	Syracuse, N. Y., ...	Term expires Nov. 2, 1882.
Charles B. Richards	Philadelphia, Pa., . . .	" " " "
John C. Hoadley, . . .	Boston, Mass., ...	" " " "
S. B. Whiting, ...	Pottsville, Pa., ...	" " Nov. 1, 1883.
J. F. Holloway, . . .	Cleveland, Ohio, . . .	" " " "
George W. Fisher, . .	St. Louis, Mo., ...	" " " "
Allan Stirling, . . .	New York City, ...	" " Nov. 7, 1884.
George H. Babcock, . .	New York City, ...	" " " "
S. W. Robinson, . . .	Columbus, Ohio, . . .	" " " "

TREASURER:

Charles W. Copeland, 24 Park Place, New York City.

SECRETARY:

Thomas Whiteside Rae, 239 Broadway, New York City.

STANDING COMMITTEES.

ON FINANCE:

WM. P. TROWBRIDGE, D. S. HINES,
LYCURGUS B. MOORE.

ON PUBLICATION:

ALLAN STIRLING, ALFRED R. WOLFF,
THOMAS WHITESIDE RAE, WILLIAM H. WILEY,
FREDERIC R. HUTTON.

ON REGULAR MEETINGS:

WASHINGTON JONES, WM. LEE CHURCH,
COLEMAN SELLERS, MATTHIAS N. FORNEY,
CHARLES E. EMERY.

ON ROOMS AND CONVERSAZIONE:

ECKLEY B. COXE, JAMES C. BAYLES,
J. C. HOADLEY, A. FABER DU FAUR,
WILLIAM CLEVELAND HICKS.

ON RAILWAY TRANSPORTATION AND HOTEL ACCOMMODATION:

S. B. WHITING, W. BARNET LE VAN,
ROBERT GRIMSHAW, THOMAS WHITESIDE RAE,
HORACE B. MILLER.

PAST OFFICERS.

HENRY R. WORTHINGTON, Vice-President, .	April 7, 1880.—Dec. 17, 1880.
COLEMAN SELLERS, Vice-President, . .	April 7, 1880—Nov. 3, 1881.
ECKLEY B. COXE, Vice-President, . .	April 7, 1880.—Nov. 3, 1881.
FRANCIS A. PRATT, Vice-President, . .	Dec. 17, 1880.—Nov. 3, 1881.
JOHN C. HOADLEY, Manager, . . .	April 7, 1880.—Nov. 3, 1881.
LYCURGUS B. MOORE, Treasurer, . .	April 7, 1880.—Dec. 2, 1881.
ALEXANDER L. HOLLEY, Vice-President, .	April 7, 1880.—Jan. 9, 1882.

AMENDED.

[*November 19th*, 1889—*November 17th*, 1891.]

RULES

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

[*Adopted November 5th*, 1884.]

OBJECTS.

ART. 1. The objects of the AMERICAN SOCIETY OF MECHANICAL ENGINEERS are to promote the Arts and Sciences connected with Engineering and Mechanical Construction, by means of meetings for social intercourse and the reading and discussion of professional papers, and to circulate, by means of publication among its members, the information thus obtained.

MEMBERSHIP.

ART. 2. The Society shall consist of Members, Honorary Members, Associates and Juniors.

ART. 3. Mechanical, Civil, Military, Mining, Metallurgical and Naval Engineers and Architects may be candidates for membership in this Society.

ART. 4. To be eligible as a *Member*, the candidate must have been so connected with some of the above-specified professions as to be considered, in the opinion of the Council, competent to take charge of work in his department, either as a designer or constructor, or else he must have been connected with the same as a teacher.

ART. 5. *Honorary Members*, not exceeding twenty-five in number, may be elected. They must be persons of acknowledged professional eminence who have virtually retired from practice.

ART. 6. To be eligible as an *Associate*, the candidate must have such a knowledge of or connection with applied science as qualifies him, in the opinion of the Council, to co-operate with engineers in the advancement of professional knowledge.

ART. 7. To be eligible as a *Junior*, the candidate must have been in the practice of engineering for at least two years, or he must be a graduate of an engineering school.

The term "Junior" applies to the professional experience, and not to the age of the candidate. Juniors may become eligible to membership.

ART. 8. All Members and Associates shall be equally entitled to the privileges of membership. Honorary Members and Juniors shall not be entitled to vote nor to be members of the Council.

ELECTION OF MEMBERS.

ART. 9. Every candidate for admission to the Society, excepting candidates for honorary membership, must be proposed by at least three members, or members and associates, to whom he must be personally known, and he must be seconded by two others. The proposal must be accompanied by a statement in writing by the candidate of the grounds of his application for election, including an account of his professional experience, and an agreement that he will conform to the requirements of membership if elected.

ART. 10. All such applications and proposals must be received and acted upon by the Council at least thirty days before a regular meeting, when the Secretary shall at once mail to each member and associate, in the form of a letter ballot, the names of candidates recommended by the Council for election.

ART. 11. Any member or associate entitled to vote may erase the name of any candidate, and may, at his option, return to the Secretary such ballot enclosed in two envelopes, the inner one to be blank and the outer one endorsed by the voter.

ART. 12. The rejection of any candidate for admission as member, associate, or junior, by *seven* voters, shall defeat the election of said candidate. The rejection of any candidate for admission as honorary member by *three* voters shall defeat the election of said candidate.

ART. 13. The said blank envelopes shall be opened by the Council at any meeting thereof, and the names of the candidates elected shall be announced in the first ensuing meeting of the Society, and also in the first ensuing list of members. The names of candidates not elected shall neither be announced nor recorded in the proceedings

ART. 14.—Candidates for admission as honorary members shall

not be required to present their claims; those making the nominations shall state the grounds therefor, and shall certify that the nominee will accept if elected. The method of election in other respects shall be the same as in case of other candidates.

ART. 15. All persons elected to the Society, excepting honorary members, must subscribe to the rules and pay to the Treasurer the initiation fee before they can receive certificates of membership. If this is not done within six months of notification of election, the election shall be void.

ART. 16. The proposers of any rejected candidate may, within three months after such rejection, lay before the Council written evidence that an error was then made, and if a reconsideration is granted, another ballot shall be ordered, at which thirteen negative votes shall be required to defeat the candidate.

ART. 17. Persons desiring to change the class of their membership shall be proposed in the same form as described for a new applicant.

FEES AND DUES.

ART. 18. The initiation fees of members and associates shall be \$25, and their annual dues shall be \$15, payable in advance. The initiation fee of juniors shall be \$15, and their annual dues \$10, payable in advance. A junior, being promoted to full membership, shall pay an additional initiation fee of \$10. Any member or associate may become, by the payment of \$200 at any one time, a life member or associate, and shall not be liable thereafter to annual dues.

ART. 19. Any member, associate or junior, in arrears may, at the discretion of the Council, be deprived of the receipt of publications, or stricken from the list of members, when in arrears for one year. Such person may be restored to membership by the Council on payment of all arrears, or by re-election after an interval of three years.

OFFICERS.

ART. 20. The affairs of the Society shall be managed by a Council, consisting of a President, six Vice-Presidents, nine Managers and a Treasurer, who shall also be the Trustees of the Society.

All past (Ex) Presidents of the Society, while they retain their membership therein, shall be known as Honorary Councillors, and shall be entitled to receive notices of all meetings of the Council

and may take part in any of its deliberations; they shall be entitled to vote upon all questions except such as affect the legal rights or obligations of the Society or its members.

ART. 21. The members of the Council shall be elected from among the members and associates of the Society at the annual meetings, and shall hold office as follows:

The President and the Treasurer for one year; and no person shall be eligible for immediate re-election as President who shall have held that office for two consecutive years; the Vice-Presidents for two years and the Managers for three years; and no Vice-President or Manager shall be eligible for immediate re-election to the same office at the expiration of the term for which he was elected.

ART. 22. A Secretary, who shall be a member of the Society, shall be appointed for one year by a majority of the members of the Council at its first meeting after the annual election, or as soon thereafter as the votes of a majority of the members of the Council can be secured for a candidate. The Secretary may be removed by a vote of twelve members of the Council, at any time after one month's notice has been given him by a majority of its members to show cause why he should not be removed, and he has been heard to that effect. The Secretary may take part in any of the deliberations of the Council, but shall not have a vote therein. His salary shall be fixed for the time he is appointed by a majority vote of the Council.

ART. 23. At each annual meeting, a President, three Vice-Presidents, three Managers and a Treasurer shall be elected, and the term of office of each shall continue until the end of the meeting at which their successors are elected.

ART. 24. The duties of all officers shall be such as usually pertain to their offices or may be delegated to them by the Council or by the Society. The Council may, in its discretion, require bonds to be given by the Treasurer.

ART. 25. The Council may, by vote of a majority of all its members, declare the place of any officer vacant, on his failure for one year, from inability or otherwise, to attend the Council meetings, or to perform the duties of his office. All such vacancies and those occurring by death or resignation shall be filled by the appointment of the Council, and any person so appointed shall hold office for the remainder of the term for which his predecessor was elected or appointed; *provided* that the said appointment shall not render him ineligible at the next annual meeting.

ART. 26. Five members of the Council shall constitute a quorum; but the Council may appoint an Executive Committee, or business may be transacted at a regularly called meeting of the Council, at which less than a quorum is present, subject to the approval of a majority of the Council, subsequently given in writing to the Secretary and recorded by him with the minutes. Absent members of the Council may vote by proxy upon subjects stated in the call for a meeting, said proxy to be deposited with the Secretary.

ART. 27. The President on assuming office shall appoint a Finance Committee and a Publication Committee and a Library Committee of five members each. The appointment of two members of each Committee shall expire at the end of each year. The Secretary shall, *ex officio*, be a member of all three Committees.

ART. 28.—The Finance Committee shall have power to order all ordinary or current expenditures, and shall audit all bills therefor. No bill shall be paid except upon their audit. When special appropriations are ordered by the Society, they shall not take effect until they have been referred to the Council and Finance Committee in conference.

ART. 29. It shall be the duty of the Publication Committee to receive all papers contributed, to decide which shall be published in the *Transactions*, and which shall be read in full at the meetings.

ART. 30. It shall be the duty of the Library Committee to take charge of the collection of all material for the Library of the Society, and to supervise all regulations for its use.

ELECTION OF OFFICERS.

ART. 31. At the regular meeting preceding the annual meeting a nominating committee of five members, not officers of the Society, shall be appointed, and this committee shall, at least thirty days before the annual meeting, send to the Secretary the names of nominees for the offices falling vacant under the rules. In addition to such regularly appointed committee, any other five members or associates, not in arrears, may constitute an independent nominating committee, and may present to the Secretary, at least thirty days before the annual meeting, all the names of such candidates as they may select. All the names of such independent nominees shall be placed upon the ballot list with nothing to distinguish them from the nominees of the regular committee, and the Secretary shall at once mail the said list of names to each member and associate in the form of a letter ballot, it being un-

derstood that the assent of the nominees shall have been secured in all cases.

ART. 32. In the election of Vice-Presidents, each member and associate may cast as many votes as there are Vice-Presidents to be elected. He may give all these votes to one candidate, or distribute them among more, as he chooses. Managers shall be voted for in the same way.

ART. 33. Any member or associate entitled to vote may vote by retaining or changing the names on said list, leaving names not exceeding in number the officers to be elected, and returning the list to the Secretary—such ballot inclosed in two envelopes, the inner one to be blank and the outer one to be indorsed by the voter. No member or associate in arrears since the last annual meeting shall be allowed to vote until said arrears shall have been paid.

ART. 34. The said blank envelopes shall be opened by tellers at the annual meeting, and the person who shall have received the greatest number of votes for the several offices shall be declared elected.

MEETINGS.

ART. 35. The annual meeting of the Society shall be held on the first Thursday in November of each year, in the City of New York, unless otherwise ordered, at which a report of proceedings and an abstract of the accounts shall be furnished by the Council. The Council may change the place of the annual meeting, and shall, in that case, give timely notice to members and associates.

ART. 36. Other regular meetings of the Society shall be held in each year at such time and place as the Council may appoint. At least thirty days' notice of all meetings shall be mailed by the Secretary to members, honorary members, associates and juniors.

ART. 37. Special meetings may be called whenever the council may see fit; and the Secretary shall call a special meeting at the written request of twenty or more members. The notices for special meetings shall state the business to be transacted, and no other shall be entertained.

ART. 38. Any member, honorary member or associate may introduce a stranger to any meeting; but the latter shall not take part in the proceedings without the consent of the meeting.

ART. 39. Every question which shall come before the Society shall be decided, unless otherwise provided by these rules, by the votes of a majority of the members and associates present, provided there is a quorum.

ART. 40. At any regular meeting of the Society thirteen or more members and associates shall constitute a quorum.

ART. 41. Unless otherwise ordered, papers shall be read in the order in which their text is received by the Secretary. Before any paper appears in the *Transactions* of the Society a copy of the paper shall be sent to the author, and, so far as possible, a copy of the reported discussion shall be sent to every member who took part in the same, with requests that attention shall be called to any errors therein.

ART. 42. The Society shall claim no exclusive copyright in papers read at its meetings, nor in reports of discussions, except in the matter of official publication with the Society's imprint, as its *Transactions*. The Secretary shall have sole possession of papers between the time of their acceptance by the Publication Committee and their reading, together with the drawings illustrating the same; and at the time of such reading, or as soon thereafter as practicable, he shall cause to be printed, with the authors' consent, copies of such papers, "subject to revision," with such illustrations as are needed for the *Transactions*, for distribution to the members and for the use of technical newspapers, American and foreign, which may desire to reprint them in whole or in part. The policy of the Society in this matter shall be to give papers read before it the widest circulation possible, with the view of making the work of the Society known, encouraging mechanical progress, and extending the professional reputation of its members.

ART. 43. The author of each paper read before the Society shall be entitled to twelve copies, if printed, for his own use, and all members shall have the right to order any number of reprints of papers at a cost to cover paper and printing; *provided*, that said copies are not intended for sale.

ART. 44. The Society is not, as a body, responsible for the statements of fact or opinion advanced in papers or discussions, at its meetings; and it is understood that papers and discussions should not include matters relating to politics or purely to trade.

AMENDMENTS.

ART. 45. These rules may be amended, at any annual meeting, by a two-thirds vote of the members present; *provided*, that written notice of the proposed amendment shall have been given at a previous meeting.

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OF THE

HARTFORD MEETING, 1881.

XIX.

PROCEEDINGS

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

FIRST REGULAR MEETING OF 1881,

HARTFORD, CONNECTICUT.

THE meeting was called to order by the President at 8 P.M., Wednesday, May 4th, in the Common Council Chamber, Hartford, Connecticut.

The following members and visitors were present:

Professor Robert H. Thurston,	Hoboken, N. J.
James Brady,	Brooklyn, N. Y.
Oberlin Smith,	Bridgeton, N. J.
George H. Smith,	Providence, R. I.
E. H. Parks,	Providence, R. I.
Stephen W. Baldwin,	New York City.
J. Sellers Bancroft,	Philadelphia, Pa.
George A. Barnard,	New York City.
James C. Bayles,	New York City.
Charles E. Billings,	Hartford, Conn.
C. H. Brown,	Fitchburg, Mass.
William Lee Church,	Hartford, Conn.
Alfred B. Couch,	Philadelphia, Pa.
David P. Davis,	New York City.
W. F. Durfee,	Bridgeport, Conn.
Professor Thomas Egleston,	New York City.
Charles E. Emery,	New York City.
Levi K. Fuller,	Brattleboro, Vt.
Edward Le Breton Gardner,	Passaic, N. J.
Alexander Gordon,	Hamilton, Ohio.
Frederick W. Gordon,	Pittsburg, Pa.
John J. Grant,	Hartford, Conn.
William Eliot Barrows,	Hartford, Conn.
William H. Harrison,	Newcastle, Pa.
Gustavus C. Henning,	Wilmington, Del.

William Hewitt,	Trenton, N. J.
D. S. Hines,	New York City.
J. C. Hoadley,	Boston, Mass.
Alexander L. Holley,	New York City.
George M. Hoimes,	Gardiner, Me.
Charles P. Howard,	Hartford, Conn.
Professor Frederick R. Hutton,	New York City.
E. D. Leavitt, Jr.,	Cambridgeport, Mass.
Lewis F. Lyne,	New York City.
William Mason,	Hartford, Conn.
Edward J. Murphy,	Hartford, Conn.
A. F. Nagle,	Providence, R. I.
William H. Odell,	Yonkers, N. Y.
E. G. Parkhurst,	Hartford, Conn.
Francis A. Pratt,	Hartford, Conn.
Thomas Whiteside Rae,	New York City.
Jackson Bailey,	New York City.
Charles B. Richards,	Philadelphia, Pa.
Francis H. Richards,	Springfield, Mass.
Joshua Rose,	New York City.
C. E. Simonds,	East Boston, Mass.
S. N. Hartwell,	Hartford, Conn.
Allan Stirling,	Drifton, Pa.
George S. Strong,	Philadelphia, Pa.
Samuel Webber,	Manchester, N. H.
W. H. Weightman,	New York City.
Joseph J. White,	Smithville, N. J.
Moses G. Wilder,	Brooklyn, N. Y.
Alfred R. Wolff,	New York City.
C. J. H. Woodbury,	Boston, Mass.
Gardner C. Hawkins,	Boston, Mass.
Horace B. Miller,	New York City.
Professor George J. Alden.	Worcester, Mass.
Thomas R. Almond.	Brooklyn, New York.
Thomas L. Churchill,	Boston, Mass.
Lycurgus B. Moore,	New York City.
Louis G. Laureau,	New York City.
Charles P. Deane,	Holyoke, Mass.
Milton P. Higgins,	Worcester, Mass.
H. A. Hill,	Boston, Mass.
Horace Lord,	Hartford, Conn.
David N. Melvin,	Linoleumville, Staten Island, N. Y.
Samuel W. Powell,	Boston, Mass.
Richard H. Soule,	Baltimore, Md.
Edward W. Thomas,	Willimantic, Conn.
Jerome Wheelock,	Worcester, Mass.
S. B. Whiting,	Pottsville, Pa.
Holbrook F. J. Porter,	New York City.
Charles Sperry,	Westbrook, Conn.
Edward N. Trump,	Wilmington, Del.

Among the invited guests present were the following:

Sidney Gilchrist Thomas,	England.
Rev. Dr. Twitchell,	Hartford, Conn.
N. C. Stiles,	Middletown, Conn.
Dwight Slate,	Hartford, Conn.
A. S. Cook,	Hartford, Conn.
W. E. Partridge,	New York City.
J. M. Allen,	Hartford, Conn.
Ex-Governor Holley,	Connecticut.
Professor Charles J. King,	Madison, Ill.
James E. Denton,	Hoboken, N. J.

President Thurston introduced Mr. De Witt C. Pond, of Hartford, President of the Common Council of that city.

MR. POND: *Mr. President and Gentlemen of the American Society of Mechanical Engineers:* In the absence of His Honor, the Mayor, whose illness renders it impossible for him to be present here this evening, the duty devolves upon me (in this case a very pleasant one), as President of the Board of Aldermen, to extend to the members of your Society a hearty greeting and welcome to this our city. In conformity to a resolution, passed unanimously by both branches of the Court of Common Council, we extend the free use of the Common Council Chambers in this building, and also the committee-rooms attached, for the meeting of your Society. That your sessions may be harmonious and profitable, and the occasion of new and pleasant relations, both among the members of the Society and the residents of our city, is the wish and desire of all.

THE PRESIDENT: In behalf of the Society, sir, I take great pleasure in expressing to you our gratitude for the privileges that you have accorded us, and to say that we have come to Hartford, among other reasons, because we expected just such hospitality. We have also come to Hartford expecting various other things. We thought it would be a safe place to come to, for we knew that life insurance was here carried to such an extent that we were absolutely safe in making a visit to Hartford. You must remember, sir, that the members of our Society are extremely busy just now. I presume no association contains so large a proportion of men who are tied to business as the members of this Society, and therefore that we have so large a gathering seems to me somewhat remarkable, and I attribute the fact very largely to that certainty which

all our members feel, that our visit to your city will be pleasant, as it is sure to be profitable.

We have also come to Hartford because of sundry business reasons—to find how certain things are done. We find this is the place where they are done well, perhaps best, and we are sure of obtaining a large amount of business from your town. And we are sure of our pay, too, if we succeed in getting that business, because we know that in a town of forty or fifty thousand inhabitants, having $32,000,000 capitalized in its banks, there will be no trouble in getting our money.

The President then addressed the Society as follows:

Gentlemen of the Society: I have the pleasure of calling to order the First Regular Meeting of 1881, and have to say that, since our last meeting, the growth of the Society has been something very remarkable. We have on our list now two hundred and fifty or two hundred and sixty names, sixty-six of which have come in since the last meeting of the Society; and when we consider the fact that the Society is, to-day, hardly a year old—for its life will date from the Annual Meeting of last November—our success in creating a nucleus, and gathering in so many men of our profession, is, I think, very remarkable indeed, and the Society is to be congratulated.

The report of the Treasurer, which will be read presently, will show you that the finances of the Society are in an equally encouraging condition. The amount that has been received from annual dues and initiation fees amounts to over $5,000, and the Treasurer has so managed things, with the assistance of the Secretary in economizing, as to have left on hand about $3,500, which he has now in bank; and the long list of new names which will be voted on this evening will supply funds for paying, I presume, all bills outstanding, so that the financial condition of the Society is as encouraging as is its membership.

We have a long list of papers, some of which will probably call for a considerable amount of debate, and for that reason it is advisable to push through the regular business that will come before us as rapidly as possible. Therefore, without any further preliminaries, I will call for the general or regular business of the meeting. The first thing in the regular order of business will be the Treasurer's report.

The Treasurer then presented his report.

TREASURER'S REPORT.

NEW YORK, May 3, 1881.

Gentlemen : I have the honor to report that I have received, in all, funds as follows :

From life membership,	$300 00
" initiation fees,	3105 00
" annual dues,	1802 00
" amount overpaid,	10 00
Total receipts,	$5217 00

Out of this amount, with the assent of the Finance Committee, I have expended as follows :

Engraving,	$19 95
Travelling (Secretary's expenses),	15 08
General expense,	342 52
Petty cash (in hands of Secretary for small miscellaneous disbursements),	57 63
Salary account (clerk hire, $13.75 ; Secretary, $875),	888 75
Printing and stationery,	361 74
Postage,	122 80
Total expenditures,	$1808 47
Leaving a balance of cash on hand,	$3408 53

Of this sum there is in bank,	$447 78
In Safe Deposit vaults, U. S. 4 per cent. bonds, face value $2,600, which cost,	2960 75
Total, as stated,	$3408 53

These bonds, I am happy to add, exclusive of interest coupons, if sold at the present market price, are worth about $50 more than cost.

There is still due the Society, from the membership, as follows :

Due from initiation fees,	$90 00
" annual dues,	266 00
Total,	$356 00

As I am informed that bills representing a considerable indebtedness for printing the Proceedings of the Meeting held last November, which have not yet been presented, will shortly be handed in to me for payment, I trust delinquents will be kind enough to make early settlement, to avoid necessity of disturbing the interest-bearing fund in the Safe Deposit vaults. I have every confidence that all, or nearly all, of the money due the Society from the membership is collectible, and will be paid.

Respectfully submitted,

LYCURGUS B. MOORE,

Treasurer.

The President: The minutes of the last meeting should properly be read as the first regular business, I presume. They have been printed, and the members have all seen them and read them, and their formal reading would simply occupy time. If there is no objection, I would propose simply to pass them, only calling for the Society's vote upon their approval.

On motion of Mr. Holley, it was agreed that the Proceedings as printed should be approved as minutes.

The President: A ballot has been sent out, and members have returned this ballot, which contains the names of a long list of candidates. The business next in order will be the counting of these ballots. There was also sent out a copy of a resolution calling for an expression of opinion from the members upon the value of the metric system. I do not remember the words of the resolution, but you have all received that ballot. That has also been returned to the Secretary, and is ready for counting. I presume the Society will order the appointment of tellers for the counting of those ballots, which will be done while other business is going on.

It was agreed, on motion of Mr. Hutton, that the chair should appoint tellers for the counting of both sets of ballots.

Messrs. Weightman, Miller, and Henning were thereupon appointed tellers.

The President: The resident Vice-President of the Society has informed the Council that there has been prepared a paper on "Standard Measurements," by George Bond. Mr. Bond is a candidate for membership, but has not yet been elected. This paper has been read by the Vice-President, and he asserts that it is a paper that will probably prove of very great interest to the Society; and it is suggested that Mr. Bond be requested, as a matter of courtesy, to present his paper to the Society without waiting for election. That is a matter that can be taken cognizance of by the Society, and it remains for it to say what shall be done. The Council has thought so favorably of the matter as to request me to present it for the consideration of the Society. The Council thoroughly approve of such action being taken, and the Society may, if it choose, take action. Is there any motion offered?

It was agreed, on motion of Mr. Charles E. Emery, that Mr. Bond be requested to read his paper, and that it be subsequently printed by the Society in its Proceedings.

Mr. Durfee: Mr. President, I have a little matter of business to which I desire to call the attention of the Society. I wish to

propose an amendment to the Rules by substituting for Article XXXVII the following:

"The Council shall have power to accept or reject any paper which may be offered for reading; and any paper read before the Society shall be printed, together with the discussion thereon, at the expense of the Society, as part of its transactions. But before such paper appears in the Transactions of the Society, a revised proof shall be sent by the Secretary to its author, and to all members who took part in the discussion, with the request that they call attention to any errors therein."

I offer this amendment, Mr. President, for this reason: that in looking over the paper on Regenerative Metallurgical Furnaces, in regard to which I made a few remarks, I find there are a number of errors of statement and of typography; and as I have not had time to look over all the papers that have been published, I fear that this may be a specimen of the whole as regards errors. I think it is quite time, for the credit of the Society, that there was some remedy for this sort of thing applied. In making these remarks, Mr. President, I do not wish to be considered as criticising any person; but I call attention to the facts as they exist, with that paper as an instance; whether the others are as bad as that, I do not know; but I think it is quite time that some means was provided for obviating such mistakes in the future.

In the paper in question the author is made to say: "In the construction of those regenerative furnaces the regenerators have been placed below ground and generally beneath the *ground*." The last "ground" should have been "furnaces;" and there are errors in capitalization. It is also stated that "when the lattice work of the stoves has been sufficiently heated the currents are reversed by opening and closing the proper valves and the cold air and gas in passing upwards through *its* lattice work,"—through *the* lattice work, it should be. As regards the discussion, I do not intend to take time to call attention to the minutiæ of the errors in its report as published; but during its progress Mr. Holley is made to say something about the "Swinnel furnace;" I do not know of any such furnace; it is *the Swindell furnace.* Then one individual is put down here as *a member*. Very fortunately for myself I was that member; and I am very glad that I am not distinguished by name, because I have been made to say things which I do not believe, and things which, if I had said, I should not have put in the shape in which they are reported. This member is made to say: "It is well known by those who are familiar

with the operation of the Siemens furnace;" I did not say anything of the kind. I said, "It is well known by persons familiar with the Siemens *puddling* furnace." The Siemens furnace generally is not open to the objection which I raised; it is only the *puddling* furnace that is subject to the difficulty named. I am made responsible for this sentence: "If that same action takes place in *this* furnace which we *shall* have under discussion." It should be "*the* furnace which we *have* under discussion." The author of the paper is made to say something in reply to my criticisms, which I am sure he did not say, for had he made the remarks attributed to him I should certainly have replied thereto, which I did not. I think if all the papers have got similar errors in them it is quite time this amendment should pass. However, I presume it must be laid over for the present.

THE PRESIDENT: That must be taken as a notice to the next meeting. It is a matter, I suppose, that requires no debate. When it comes up as a formal motion debate will be in order.

MR. CHURCH: Mr. President, it is necessary for me to state that a letter was placed in my hands for the benefit of the Society by Mr. Allen, President of the Hartford Steam Boiler Inspection and Insurance Company, in which he extends the freedom of their offices and laboratory to the members of the association. Owing to unpardonable negligence on my part I have not the letter with me. I beg to apologize to the Society, and present his very courteous request and invitation verbally.

THE PRESIDENT: I have no doubt Mr. Church's presentation of the case will be quite as acceptable as Mr. Allen's. And that is a reminder to me also that we have received to-day a letter from the President of Trinity College extending an invitation to all the members to visit that college. And that, by the way, is a matter the Local Committee can take into its programme if it choose. Trinity College will be found a very interesting place to visit. And I hold in my hand also a letter from General Franklin, the Vice-President of the Colt Patent Fire-Arms Manufacturing Company, in which he says: "This company will be very glad to have the members of the Society visit the factory individually or collectively as they may prefer; and its officers will be pleased to tender them any service in their power."

Both of these matters will properly be referred to the Local Committee, which is managing all these matters. If there is no objection they stand so referred.

Mr. Pratt: I suppose it was taken for granted that the Society would visit these places. I would extend an invitation to visit our establishment if the invitation is needed.

The President: This whole matter was in the hands of the Local Committee, and the arrangements for the visit of the Society as a body properly come under its management, and I presume it has the programme arranged. But the invitations coming in as they do of course will answer as individual invitations as well as official invitations from any local committee. The Local Committee will take such action as it sees fit in regard to the invitations. In the meantime members have the privilege of going and coming of course as they choose.

The President: The tellers report that they have counted the ballots referring to the resolution relative to the metric system. The resolution was

"*Resolved*, That the Society deprecate any legislation tending to make obligatory the introduction of the metric system of measurement into our industrial establishments; also, that the Secretary be instructed to communicate the sentiments of this resolution to any one concerned in procuring such legislation; and further, that a copy of this resolution be sent to the Anti-metric Society, of Cleveland, Ohio."

The whole vote was 135, of which 111 are voting aye and 24 no. The resolution as passed by the Society will be entered upon the minutes, and the Secretary will send a copy of it to the Anti-metric Society, of Cleveland, Ohio, as ordered.

The ballot list of proposed members has been examined; the tellers report that the whole vote amounts to 145, and there are no negatives. The whole list as printed is elected. If any of these gentlemen are in the house the official notification can be handed them by the Secretary at any moment, and they become members at once on interviewing the Treasurer. I will read the list.

MEMBERS.

Captain John Ericsson,	New York City.
Erastus W. Smith,	New York City.
J. Vaughan Merrick,	Philadelphia, Pa.
Samuel McElroy,	Brooklyn, N. Y.
James H. Alexander,	New York City.
Stephen E. Babcock,	Troy, N. Y.
George H. Barrus,	Boston, Mass.
John W. Cloud,	Altoona, Pa.
Alfred Colin,	New York City.
James E. Denton,	Hoboken, N. J.

W. J. M. Dobson.	New York City.
John Fritz,	Bethlehem, Pa.
Robert Forsyth,	Chicago, Ill.
H. P. Gregory,	San Francisco, Cal.
S. N. Hartwell,	Hartford, Conn.
John T. Henthorn,	Providence, R. I.
William Cleveland Hicks,	New York City.
C. C. Hill,	Chicago, Ill.
Joseph J. Illingworth,	Utica, N. Y.
Daniel N. Jones,	Johnstown, Pa.
Henry C. Jones,	Wilmington, Del.
William R. Jones,	Pittsburgh, Pa.
Louis G. Laureau,	New York City.
William A. Leavitt,	Philadelphia, Pa.
Alexander Miller,	New York City.
Charles H. Morgan,	Worcester, Mass.
James Park, Jr.,	Pittsburgh, Pa.
Theodore H. Risdon,	Mount Holly, N. J.
Oberlin Smith,	Bridgeton, N. J.
Horace S. Smith,	Joliet, Ill.
Henry F. Snyder,	Watsontown, Pa.
Frank G. Tallman,	Providence, R. I.
James Thomas,	Catasauqua, Pa.
Samuel T. Wellman,	Cleveland, Ohio.
William Oliver Webber,	Pullman, Ill.

ASSOCIATE.

W. H. Bailey,	New York City.

On motion of Mr. Gordon the meeting then adjourned till the following day at 10 o'clock A.M.

Second Day's Proceedings.

The meeting was called to order at 10 A.M.

The President: Some gentlemen present, who are members, find themselves here almost total strangers. I would like to suggest to those gentlemen that they make the acquaintance of the Secretary, who is supposed to know every one in the Society, and whose duty it is to effect such acquaintance among those who are not known to each other. If those gentlemen will introduce themselves to any one of the officers of the Society, an endeavor will be made to put them in communication with other gentlemen, and so start a little round of acquaintance that will be pleasant to them. The new members coming in are advised to take the same course,—make the acquaintance of the officers of the Society, and ask them to introduce them to such members present

as they may most desire to become acquainted with. In that way I think the range of acquaintance will be very greatly and very pleasantly extended.

When we adjourned last night, the discussion of the two papers read during the evening was interrupted; and I am reminded by the fact that the course of discussion last night was a little irregular to advise the gentlemen to adhere to the usual rule in such cases, which provides ordinarily that the writer of a paper listens to the discussion, and, when the discussion is concluded, he will close the discussion by a rejoinder to the points made; and, by pursuing that course, gentlemen will find business very much facilitated.

Gentlemen who propose to take part in the debate will please do so promptly, and say what they have to say as concisely as possible. When that side of the discussion is closed, the writer of the paper will be expected to defend the points that he has made and answer questions that may be asked him.

That is the usual course of discussion, and I presume it will be found advisable to follow it in these proceedings.

The President read a letter from Mr. Bissom, of Hartford, inviting the members of the Society to visit his office and examine his invention for announcing stations.

Mr. Lyne: There is a fact that I would like to call attention to, and it is this. At our last meeting there were a number of new members present who were not acquainted. They came into the meeting strangers, and they went away strangers; and it occurs to me that we ought to have an introducing committee, for instance, composed say, of two members, who are well acquainted, and who will perform their duties in a creditable manner to make everybody acquainted. It seems to me that our progress as an association depends very largely upon the cultivation of the social element; and I merely suggest this for the consideration of the members, hoping that it will have the proper treatment, and that in the future we may take the proper course to make our new members acquainted, that they may enjoy themselves and that good results may come out of it. I would like to hear the views of other members in relation to this matter. I mention it at this time in order that we may take some action before it is too late.

The President: You have heard the remarks, gentlemen; is there anything more to be said on that matter—any motion to be made? Members who have come in, who are strangers here, should

always understand that if they make themselves known to the officers of the Society, we shall always be glad to make them acquainted with members of the Society whose acquaintance they think to be especially desirable.

Mr. Holley: I wish to give notice that at the next meeting I shall move the following amendment to Article XIII, regarding the election of members. I shall move that that article be amended so as to read as follows:

"The said blank envelopes shall be opened by the Council at any meeting thereof, and the names of candidates elected shall be announced in the first ensuing meeting of the Society and also in the first ensuing list of members."

The object in making this proposition is not to give the Council additional power. Of course the Society is and should be jealous about that. The Council cannot get any additional power under that. All they can do is to open the ballots. The object is to have an early notification of the election of new members. For instance, if the Council at the meeting previous to this general meeting of the Society had opened the ballots, then all the new members would have been here. But they now feel a sort of hesitation in coming until they know whether they have been elected or not. That is a matter I think that ought to be changed.

The President: It is a matter that requires no discussion at present. It will come up in order at the next meeting.

Mr. Weightman: I would like to ask whether the Council have done anything towards obtaining a charter.

The President: Nothing has been done in that direction. The matter has been discussed, but no conclusion has been reached.

Mr. Durfee: I think the proposition of Mr. Holley a very excellent one indeed; if it could be put through at this meeting, it will have operation for the members who are elected previous to the next meeting, and they would be duly notified so that they would be present at that time. If it could be acted upon under a suspension of the rules, I would move a suspension of the rules, and that it be taken up for consideration now.

The President: The Society, I presume, has the power to suspend its own rules as in all other societies.

Mr. Hutton seconded the motion for the suspension of the rules, which was agreed to.

The resolution proposed by Mr. Holley was then adopted.

The meeting then adjourned until 7.30 P.M. the following day.

THIRD DAY'S PROCEEDINGS.

The meeting was called to order at 7.30 P.M.

THE PRESIDENT: I hold in my hand a letter from the Superintendent of the New York and New England Railroad, dated Hartford, May 6th, 1881, as follows:

"TO THE PRESIDENT OF THE SOCIETY OF MECHANICAL ENGINEERS.

"DEAR SIR: General Wilson, President of the New York and New England Railroad, has directed me to present to your Society his compliments, and to offer them the civilities of his road. He has also directed me to place at the disposal of your Society a special car, upon which they may make the trip from this city to Boston and return. Should you deem it best to accept this courtesy, be kind enough to inform me this evening. We can arrange to run the special at 9.05 A.M. or 1.40 P.M. to-morrow.

"Very respectfully yours,
"J. C. RAWN,
"Superintendent."

If any gentlemen would like to take advantage of that very kind offer, they will please notify the Secretary as promptly as may be, so that it may be ascertained how many would like to go, if any, and if the number is sufficient to justify the putting on of a special car.

THE PRESIDENT: It would have been proper before commencing the reading of papers to have transacted any business in order, but owing to the fact that there were comparatively few members here, and the fact that one who had promised to introduce a resolution of thanks to the people who have been so kind to us in Hartford was absent, the matter was deferred. But if no objection is made I will bring it in as a matter of business now. I presume it will be proper, as the first business in order, to give some consideration to the claims that our friends have made upon us here by their hospitalities, and a resolution of the Society in that direction would certainly be proper. If you are ready for it, Mr. Moore is ready to present it.

MR. MOORE: *Mr. President and Gentlemen:* I have been requested by the Council to give expression to the feelings of obligation under which the members of the Society have been placed through the courtesy of the citizens of Hartford. By what theory of selection this duty has been assigned to me, I do not know, unless possibly it is my connection with the Society as one of its

officers. The members have been so accustomed to turning over bills and obligations to me for payment that perhaps they may have asked in this case from mere force of habit.

But I must remind you that a good paymaster always wants some kind of voucher, and you, as members of the Society, are the auditing committee, and must express your approval before this debt which we owe the citizens of Hartford can be paid. Not being a debt of the pocket it cannot be paid with money. I therefore beg to offer the following resolution:

"*Resolved*, That the thanks of this Society are due and are hereby tendered to his Honor the Mayor, and to the Common Council of Hartford, for the use of audience and committee-rooms; to the manufacturers of the city and vicinity for courtesies extended, and also to the Local Committee."

The resolution was agreed to.

The President: The Secretary will enter the resolution on the minutes and forward copies.

The President read a note from the Superintendent of the New York and New England Railroad Company, requesting that such members of the Society as wished to visit Boston would call at the office of the company and obtain the requisite passes. He continued:

There are several other papers on the list, which were put at the end by the Committee on Publication. The Secretary says Professor Robinson's papers are very valuable, but so purely mathematical that in his absence their discussion could hardly take place; and it has been thought advisable to accept the suggestion of the Committee on Publication, and so defer the reading of these papers until the next succeeding meeting. If the authors prefer to have it otherwise they can be read by title, and if printed, will come into the hands of the members, and will be discussed.

The Secretary read the papers referred to by the President, by title, as follows:

"Latest Methods of Submarine Telegraph Work," by Mr. Rae.

"Counterbalancing of the Reciprocating Parts of Steam-engines and other Machinery," by Professor Robinson.

"A Rational System of Piston Packing," by Professor Robinson.

"Rolled Cast Steel Car Wheels," by Mr. Reese.

"Comparison of Different Types of Engines," by Mr. Hague.

Mr. Pratt: I wish to say to the members of this Society who visited our works to-day, I intended to give them all a little

pamphlet, which I have with me now; it was forgotten at the time; and we will leave them where they can take them. It is on "The Teeth of Spur Wheels." They have all examined the apparatus to-day, and I suppose appreciated it.

Mr. Stirling: Mr. President, it has occurred to me to suggest to the Council the desirability of having the papers printed, and sent to the members about a month before the time of the meeting at which they are to be read. Some of these papers are lengthy, and deep, and require some study; and it is hardly fair to debate them at sight, on first reading, first presentation.

The President: That matter has been under consideration. In the Institute of Mining Engineers they have succeeded in the adoption of that plan to a considerable extent, and papers now are sent out in advance of their reading, I think; are they not, Mr. Holley?

Mr. Holley: Occasionally; yes.

The President: Occasionally. In our own case papers have usually come in so late that it would be entirely out of the question; in fact it would be doubtful whether they would then arrive in time to come before us at all. Some of the most valuable papers presented to us at this meeting have come in latest; but in other cases it would be perfectly practicable; and it seems to me, and to many members who have talked with me on the subject, that it would be a useful thing to do; and I have not any doubt that after our meetings get fairly established, and members get accustomed to the expectation of assembling at definite periods of the year, and to the idea of being prepared with papers, that we shall be able to carry out that system. I am sure with many papers we have had before us at this meeting, it would have been vastly advantageous to all to have had them handed to the members in time to allow of their study before coming up for debate. In one cr two cases members have brought in papers at this meeting, and have said that they would very much prefer that the papers be passed without debate, and have the discussion come up at a succeeding meeting, after members have had the papers in their hands, and have had an opportunity to study and digest them. I presume, however, it is too soon to take definite action in the matter; but, it is certainly worthy of consideration.

Mr. Holley: I would like to suggest on that subject that it has been found in various societies that any rigid rule requiring papers to be handed in in time to be printed, would shut off some of the most valuable papers. It certainly has been so in the Institute

of Mining Engineers. I presume Mr. Stirling's wish, which is a very correct one, would be just as well accomplished by having the papers discussed at the next meeting following the meeting when they are read.

Mr. Stirling: I should think that would be a very satisfactory arrangement, that those which were not before us previous to the meeting should be laid away for discussion at the next meeting.

Mr. Harrison: I suppose it might be presumed, Mr. President, that after we approach perfection in these matters, a person will go to the trouble to work up a subject pretty thoroughly, and work out his paper on it. It might be best, perhaps, to post a person on that subject so no special discussion would be necessary. Perhaps not at present, but one of these days.

The President: When the Society adjourns this evening, it adjourns to meet at the call of the Council. Our rules require that between successive annual meetings, which must occur at the headquarters in New York, there be two meetings, which will be held usually at other places. The next meeting, therefore, will come at some time between now and November; and to adjust the matter properly, it will probably naturally come some time in August, during the hot weather. There has been some discussion among the members, but no formal action has been taken by the Council,—they are waiting for the opinion of members as to what shall be done. But there seems to be an inclination to find some pleasant, cool, interesting spot, about mid-summer, where we can have a pleasant, comfortable meeting. Altoona has been spoken of, and one or two members have promised to see what can be done there. One or two members have spoken of Boston as being a place where we could find a warm reception, a pleasant time, and matters of special interest at the time of the opening of the New England Fair,—that would be rather late in September. But those subjects can be thought of by members, and I would suggest that those members who are interested specially in that matter consider the subject carefully and send a note to either of the officers, so that any suggestions they would like to make may come before the Council early. But nothing definite has yet been done—no definite action has been taken. It simply looks probable that we shall have a meeting called by the Council at about perhaps the middle of August—at, I should say, early in August.

The meeting then adjourned, subject to the call of the Council.

PAPERS

OF THE

HARTFORD MEETING, 1881.

XX.

A RATIONAL SYSTEM OF PISTON PACKING.

BY S. W. ROBINSON, C. E., PROF. MECH. ENG., STATE UNIVERSITY, COLUMBUS, OHIO.

THE primary object of a good piston packing is evidently to prevent the working fluid from escaping past the piston. The sole object of the present paper is to investigate how this can be done most efficiently; leaving out of the question all such uses of packing as making it serve the purpose of a carriage for support of the piston, etc.; except, perhaps, a reference to this matter of carriage support—in that it seems desirable to have a carriage for the piston, especially in horizontal engines, and where the cylinder has been bored out larger than the piston to correct irregular wear after some use. But a glance at the matter will show that for the packing to serve as carriage, it must be very wide in order to present the necessary wearing surface for durability, and that where the packing is wide, the rims of the piston must be correspondingly narrow, and be accordingly unfitted for bearing the weight of the piston; and that the flexibility of the packing rings will allow them to take a somewhat irregular form, thus tending to wear the cylinder out of round; and that the devices for setting out the rings may change in intensity of action so as to cause the piston to ride upon the cylinder at some side, causing relatively rapid wear of the narrow rims of piston; and that the piston-rod will thus be brought into wearing contact against its packing glands; and that in view of these facts it may be desirable to introduce a groove containing a solid, stout half ring about at the middle of the face of piston to serve as a carriage, with the piston properly jacked upon it; and, finally, that all these devices may be replaced by a considerable breadth of piston rims for support of piston, and narrow packing rings under outward pressure to prevent escape of working fluid. This is all the more evident when we reflect that the piston may be made light—hollow, if necessary, for this end, and the weight added to the cross-head or the connecting-rod—in which event the piston and cylinder will be subjected to the least possible wear, and thus

scarcely need re-boring within the life of the engine. It is evident that this result is preferable to that where the cylinder frequently gets out of form to the extent necessitating repeated re-boring, when the lifetime of the engine will be more or less afflicted with "piston blowing."

Hence it is proposed here to adopt as light a piston as possible, to use very narrow packing rings, and wide rims on the piston, especially for horizontal engines. The simplest possible packing consists of a single ring split upon one side. The simplest piston is one in a single piece, possibly cast hollow for securing desirable form. Hence, a single ring, sprung into a groove turned on the face of such a piston, combines the elements of simplicity. Two or more rings may, however, be used on a single piston, each sprung into its own groove.

We will now investigate the single ring packing. It will appear afterwards that to set the ring out by pressure of the working fluid transmitted to the under side of the ring through holes, causes excessive frictional contact of the ring upon the surface of the cylinder. It would seem desirable, therefore, to give the ring its pressure of contact by means of the inherent stiffness of the ring itself, by turning it larger than the cylinder bore.

A ring split upon one side and pressing outward with a constant pressure, as it evidently should for greatest efficiency, will necessarily be thicker at points opposite the split, as shown in Fig. 2.

FORM OF PACKING RING.

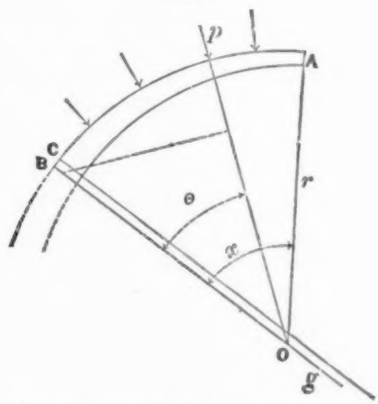
To find the law of varying thickness let A B, Fig. 1, represent a piece of such a packing ring, the end or split being at A. Then the normal pressure of the ring against the cylinder along the outside of A B will be due to a flexural moment, or resistance to bending, of the section of ring at B. This, according to the theory of flexure, will be

$$\text{Flexural moment at B} = \frac{EI}{\rho} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where E is the coefficient of elasticity of the material of the ring, I the moment of inertia of the section of ring at B, and ρ the radius of curvature due to the bending. This radius is not to be confounded with the radius of the ring. To define it clearly, draw a line parallel to B O and very near to B, as at C. Suppose these lines fixed to the ring, and parallel before flexure. After

flexure these lines will intersect at some point more or less remote from B, according to the amount of bending. The distance from B to the point of intersection is ρ. It is evident that if the ring is of circular form before flexure, and also after, ρ will be of constant length for all parts of the ring. As the ring is most conveniently made of circular form, and as it will be used in a circular cylinder, it is desirable that the thickness shall so vary as to realize this and make ρ constant.

FIG. 1.



According to the theory of flexure the flexural moment must equal the moment of the applied forces. This latter will, in the present case, be the resultant moment of all the normal pressures on A B. Let P represent the normal pressure per unit of surface of the ring against the cylinder, and if b be the breadth of ring, the pressure per unit of length of ring will be Pb, and for a length $rd\theta$, the pressure will be $Pb.rd\theta$. The moment of this element will be $Pbrd\theta.r \sin\theta$ with reference to an origin of moments at B, where the origin is taken for eq. (1). The integral of this, for θ varied between the limits x and o, will give the total moment of the forces acting on the part A B, or

$$\frac{EI}{\rho} = \int_o^x Pbr^2 \sin\theta \, d\,\theta = Pbr^2 (1 - \cos x)$$

$$= 2\,Pbr^2 \sin^2\frac{x}{2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

P being regarded as the effective pressure, and constant.

Let z be the thickness of the ring at B. Then

$$I = \tfrac{1}{12}\,bz^3$$

and

$$\frac{EI}{b\rho} = \frac{Ez^3}{12\rho} = 2\,Pr^2 \sin^2\frac{x}{2}.$$

At the middle of ring, or where A B = 180°, we have

$$\frac{Ez_1^3}{12\rho_1} = 2\,Pr^2$$

whence
$$\frac{z^3}{\rho} = \frac{z_1^3}{\rho_1}\sin^2\frac{x}{2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

an equation for the whole ring, and giving the relation between z, ρ and x; upon any one of which we may impose arbitrary conditions, the other two remaining mutually variable. If we make z constant, as would follow from turning the ring of uniform thickness, ρ would necessarily vary with x and be infinite at the split when the ring is in use. This would indicate no bending of the ring near the split, and hence the tip end would here bear heavily upon the cylinder, with a probable space of no contact for some distance back. The only useful supposition for x is that it shall remain variable. As pointed out above, considerations of convenience make ρ constant, and hence necessarily equal ρ_1. Hence we have

$$z^3 = z_1^3\sin^2\frac{x}{2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

for the final equation expressing the law of thickness, z, of ring.

It appears from the equation that the relation between z and z_1 is independent of the radius of the cylinder, and of the pressure of ring against the surface of cylinder. Hence, taking $z_1 = 1$, a table can be computed for the values of z, by which a ring for any cylinder can be drawn. Then, when the particular value of z_1 for any practical case is found to be other than 1, the values of z are to be proportionately modified. Making $z_1 = 1$, we find the following

TABLE OF VALUES FOR LAYING OUT THE RING.

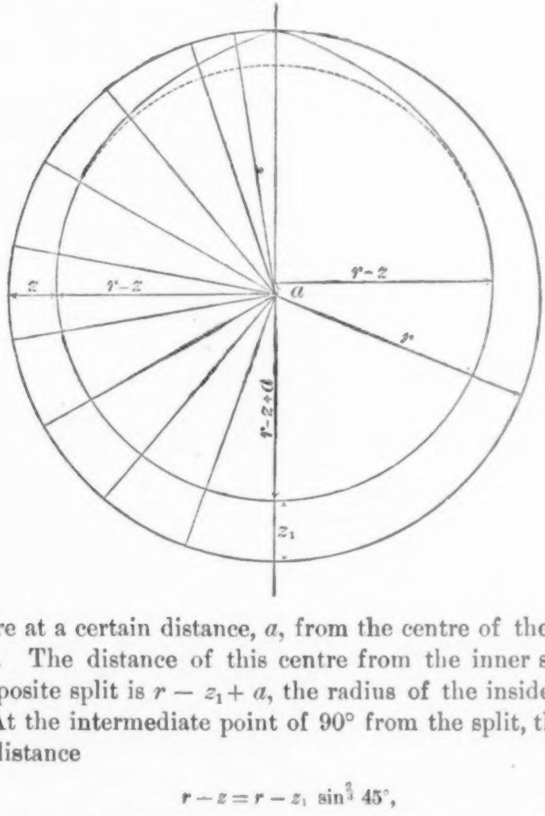
$\frac{x}{2}$	$\sin\frac{x}{2}$	$z = \sin^{\frac{2}{3}}\frac{x}{2}$
5°	.087	.197
10	.174	.311
20	.342	.489
30	.500	.630
40	.643	.745
50	.766	.837
60	.866	.908
70	.940	.960
80	.985	.990
90	1.000	1.000

By these values the ring is drawn in Fig. 2 to a scale. The out-

side is taken, the circle of the cylinder into which the ring is supposed to be compressed when ready for service.

From this drawing it is found by placing one leg of a pair of dividers a distance, a, toward the split of the ring, and swinging the other leg about near the inside edge of ring, that about $\frac{2}{3}$ of the inner line of ring lies almost exactly on the arc of a circle, and with

FIG. 2.



its centre at a certain distance, a, from the centre of the exterior of ring. The distance of this centre from the inner surface of ring opposite split is $r - z_1 + a$, the radius of the inside circle of ring. At the intermediate point of 90° from the split, this centre is at a distance

$$r - z = r - z_1 \sin^{\frac{2}{3}} 45°,$$

from the inner surface of ring, which is also the radius of the inside circle. Making these distances equal, we find

$$r - z_1 + a = r - z_1 \sin^{\frac{2}{3}} 45°$$

$$a = z_1 \ (1 - \sin^{\frac{2}{3}} 45°)$$

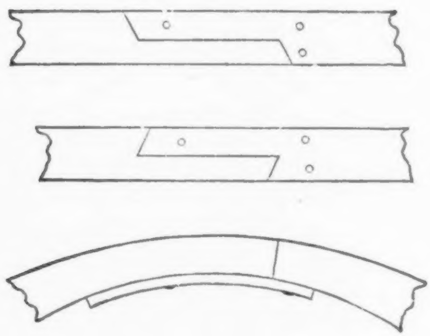
$$a = 0.206 \ z_1 \text{ nearly} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

If it be assumed that this circle gives the inner form of the ring with a sufficient degree of exactness, for about $\frac{2}{3}$ its extent, the

ring can be formed in a turning lathe complete, except for the $\frac{1}{3}$ of inside surface, a half of which is on each side of split. This could be dressed off subsequently. The lathe-work would then most conveniently consist of mounting upon a face plate, or in a chuck, a cylindric shell long enough to make several rings, with a stay of cross-arms. The inside is to be first bored deep enough for several rings, and then the ring offset to the eccentricity $a = .206\ z_1$, and the outside turned off. The rings are then to be cut off with such breadth as desired. The eccentricity is observed to be a little over $\frac{1}{5}$ the greatest thickness of ring.

In practice the tip ends of the ring at the split should be lapped a little, instead of being as shown in Fig. 2. A suitable way of doing this is shown in Fig. 3. For a perfect and complete stop to

FIG. 3.

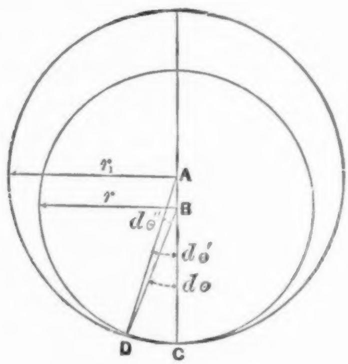


the working fluid by ring, a strip should be riveted upon the under side of one tip, so as to lie close to the under side of the other tip when in place, as shown. Otherwise, when the ring has worn so as to separate a little at the tips, the working fluid may enter between the tips at one side, pass under the ring to the opposite side, thence out and escape. It is evident that for thus lapping the tips, the thickness, to that extent, should be constant, and not vary as in Fig. 2. To adjust the bearing of the lapping part against the cylinder, it is plainly necessary to dress off a trifle of the overlapping tip outside, to compensate for the increased stiffness due to the deviation from Fig. 2 to a uniformity of thickness. The extent to which the tips may be made to overlap, as in Fig. 3, for a given excess of diameter at which the ring may be turned, will be considered subsequently.

OUTWARD PRESSURE OF RING.

It is evident that the outward pressure of the ring against the inside surface of cylinder will depend upon the diameter of the ring, its thickness, and the excess in diameter given to it when turned.

FIG. 4.



Let A be the centre of the ring when turned to the radius r_1, and B the centre when it is compressed into the cylinder of radius r. Let D C be a small part of that ring, so that

$$DC = r d\theta = r_1 d\theta' = \rho d\theta'' \quad . \quad . \quad . \quad . \quad (6)$$

ρ being the radius of curvature due to the bending or compressing of the ring, to change it from r_1 to r, Fig. 4, makes it evident that

$$d\theta'' = d\theta - d\theta' . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

By aid of (6) and (7) we may write

$$\frac{d\theta - d\theta'}{d\theta} = \frac{r_1 - r}{r_1} = \frac{d\theta''}{d\theta} = \frac{r}{\rho}$$

whence

$$\frac{r}{\rho} = \frac{r_1 - r}{r_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

But as a constant value of ρ has been adopted, $\rho = \rho_1$, and hence, combining (8) with the equation preceding (3), we have

$$\frac{E z_1^3}{12} \cdot \frac{r_1 - r}{r_1 r} = 2 P r^2$$

whence the effective normal pressure of ring per square unit is

$$P = \frac{E}{24} \cdot \frac{z_1^3}{r^3} \cdot \frac{r_1 - r}{r_1}.$$

Hence it appears that the intensity of pressure of ring upon cylinder will vary directly as the cube of the thickness, and nearly as the excess of diameter at which the ring is turned.

This excess in external diameter, of ring over the cylinder bore, is arbitrary. But in assuming the excess, probably no one would take the mean diameter of ring, inside and outside, less than that of the cylinder bore. A little consideration would probably fix it larger. If we adopt such a relation that $\frac{2}{3}$ of the ring will be outside the groove when first sprung into it, we have

rad. of piston, $r = r_1 - \frac{2}{3}(r_1 - \text{inside rad.})$.

But inside rad. $= r_1 - z_1 + a = r_1 - .794\, z_1$.

$$\therefore r = r_1 - .529\, z.$$

$$\therefore \frac{r_1 - r}{r_1} = \frac{.529\, z_1}{r_1} = .5 \frac{z_1}{r} \text{ nearly.}$$

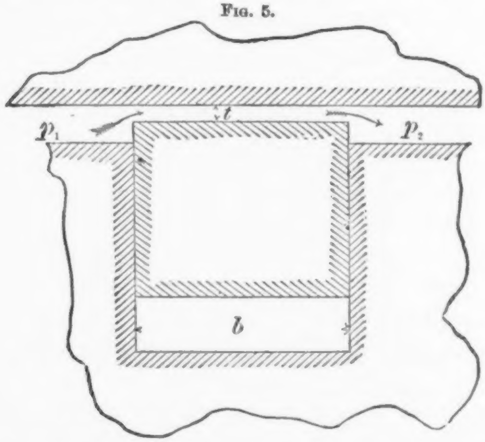
Hence, hereafter calling P the absolute pressure, and p_2 the back pressure,

$$P - p_2 = \frac{E\, z_1{}^4}{48\, r^4}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

TENDENCY OF FLUID TO LIFT THE RING.

In a thin space between the ring and cylinder, the fluid will flow, as shown in Fig. 5, from p_1 to p_2, the frictional resistance of an inelastic fluid like water, in the space t, hindering the flow, and giving cause for pressure upon ring; and in elastic fluids, like

FIG. 5.



air, both the frictional resistance and expansive pressure in the space t, between ring and cylinder, will have a combined effect to press the ring back, and to increase the space for escape.

1. *Case of Ordinary Water Pump.*— Here the density does not sensibly change between p_1 and p_2. In this case the velocity of flow will be constant within the space t, and the reduction of pressure will be in two parts; first, for acceleration at entry to the space, and second, for frictional resistance along the space. According to an article which I have lately published in *Van Nostrand's Engineering Magazine* (see page 372),

$$h' + h'' = h = \frac{v^2}{2gu^2} + \frac{fsl}{a}\frac{v^2}{2g} = \frac{p_1 - p}{\delta} + \frac{p - p_2}{\delta},$$

or
$$p_1 - p = \frac{\delta v^2}{2gu^2} \quad . \quad . \quad . \quad . \quad . \quad (10)$$

for entry, and
$$p - p_2 = \frac{\delta fsl}{a}\frac{v^2}{2g} \quad . \quad . \quad . \quad . \quad . \quad (11)$$

for the space t, in which v is the velocity in the space, u the coefficient of velocity for the orifice of entry, here about 0.85; δ the weight per unit of volume of the flowing fluid; a, the sectional area, transverse to the flow, of so much of the space t as is considered; s, the perimeter of the same section; f, the coefficient of friction of the fluid against the ring or cylinder; $l = b$, the length in the direction of motion of the flowing sheet, or breadth of ring; and p, the absolute pressure of fluid just within the entrance to the space t. The pressure will gradually decrease, from p to p_2, as the fluid passes along the space t. If x be taken for the distance from the p_2 side of ring, we have

$$p - p_2 = \frac{\delta fsx}{a}\frac{v^2}{2g} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

where p will now be the pressure of fluid in the space t at the distance x. This pressure tends to push the ring back, and it being variable, the total amount of it for the whole breadth, b, of ring, and for a unit's length along the ring, will be,

$$\mathrm{P}b = \int_0^b p\,dx = \int_0^b \left(\frac{\delta fs}{a}\frac{v^2}{2g}x + p_2\right)dx \quad . \quad . \quad . \quad . \quad (13)$$

$$= \frac{\delta fs}{a}\frac{v^2}{2g}\frac{b^2}{2} + p_2 b = (p - p_2)\frac{b}{2} + p_2 b$$

by eq. (11). Hence $\dfrac{\mathrm{P}}{p_2} = \tfrac{1}{2}\left(1 + \dfrac{p}{p_2}\right) \quad . \quad . \quad . \quad . \quad . \quad (14)$

and numerically for

$\frac{p}{p_2}$	$\frac{\mathrm{P}}{p_2}$	
= 2	= 1.5	
4	2.5	
6	3.5	(A)
8	4.5	
10	5.5	

an expression giving in terms of the pressures of fluid at opposite edges of packing ring, the ratio of the total pressure tending to push the ring away from the cylinder to the pressure at the exhaust side of piston. These pressures are per square unit of area absolute. A noticeable fact is that this ratio is independent of the breadth of ring, of the density of fluid, and of the coefficient of friction, except so far as p may depend upon them. That p will not coincide with p_1 is evident from (10).

To find the relative proportions of fall of pressure due to (10) and (11), we may take their ratio. Hence

$$\frac{p_1 - p}{p - p_2} = \frac{a}{fsbu^2} = \frac{t}{2fbu^2} \quad . \quad . \quad . \quad . \quad . \quad (15)$$

in which

$$\frac{a}{s} = \frac{t}{2},$$

since for a given extent, k, along the ring

$$a = kt, \qquad s = 2k.$$

Hence the fall of pressure varies only with t and b, the thickness of space and length of flow between ring and cylinder. But it will, of course, be desirable that t be very small, far within the hundredth part of an inch. If, for example, it be taken in thousandths of an inch, we have for $f = .006$, $b = .5$, and $u^2 = .7$.

	$\frac{p_1 - p}{p - p_2}$ =	
t = 1 thousandth	.238	
2 "	.476	(B)
4 "	.952	
8 "	1.904	
1 hundredth	2.381	

Hence, with a thickness of space t of a little over 4 thousandths of an inch, half the fall of pressure from p_1 to p_2 will be consumed in entering the fluid into the space t. As the thickness of this space thus appears to be an important element in determining $\frac{P}{p_2}$ and consequently the thickness, z_1, of ring, the latter diminishing with P, it becomes desirable to determine the admissible limits of t. This is to be done by aid of the equation just preceding (10), whence the velocity of escape is found by

$$v^2 = \frac{p_1 - p_2}{\delta} \cdot \frac{2g}{\frac{1}{u^2} + \frac{2fb}{t}} \quad . \quad . \quad . \quad . \quad (16)$$

The percentage of the whole piston displacement in the volume escaping past the piston will be

$$\frac{q}{Q} = \frac{v \times \text{area of whole space } t}{\text{piston displacement}} = \frac{v \times 2\pi rt}{\pi r^2 V} = \frac{2vt}{rV}$$

where V is the piston speed in ft. per sec.

Combining this with (16) by eliminating v, and we obtain,

$$\left(\frac{q}{Q}\right)^2 = \frac{\frac{8gt^2}{r^2V^2\delta}\left(\frac{p_1}{p_2} - 1\right)p_2}{\frac{1}{u^2} + \frac{2fb}{t}} \quad . \quad . \quad . \quad . \qquad (17)$$

If it is desired to take account of the effect of the motion of piston and of packing-ring upon the actual velocity of fluid escape, we find the latter to be,

$$v - \frac{V}{2}$$

and then the first number of (17) should be

$$\left(\frac{q}{Q} + \frac{t}{r}\right)^2$$

This may be put in a more convenient form by factoring and tabulating a part. Thus, if $d = 2r$ = diameter of piston,

$$\frac{q}{Q} + \frac{2t}{d} = \frac{1}{Vd}\sqrt{\frac{p_1}{p_2} - 1} \text{ (Tab. val.)} \quad . \quad . \qquad (18)$$

where, for water-pump rings of a width $b = .5$ of an inch, and for

$t =$	1	2	5	10	thousandths of an inch.
Tab. val. =	.0057	.0104	.0480	.1092.	(C)

For these values p_2 is taken at one atmosphere.

To obtain an idea of the practical values of (18), we have for a piston speed of pump piston of $V = 2$ feet per sec.,

$d = \frac{1}{2}$ ft., or $Vd = 1$, and $\frac{p_1}{p_2}$ in atmospheres.

For t in inches =	1	2	5	10 thousandths.	
$\frac{p_1}{p_2} = 2, \ldots . \frac{q}{Q} =$	.005	.010	.048	.109	(D)
$\frac{p_1}{p_2} = 8, \ldots . \frac{q}{Q} =$	.015	.027	.127	.288	

the term $\frac{2t}{d}$ being inconsiderable.

As the maximum admissible leakage of piston would probably be assumed from 1 to 5 per cent. of piston displacement, it appears that the greatest admissible thickness of space, t, is about 2 thousandths of an inch in pumps.

This thickness, compared with the table of values for eq. (15), indicates that we may count on

$$\frac{p_1 - p}{p - p_2} = \text{not greater than } o \text{ to } .5,$$

or the fall of pressure on entering the space t should not exceed a half of the fall due to passing the ring; or it should not exceed a third of the total fall from p_1 to p_2. In other words, the fall of pressure, $p - p_2$, in passing the ring, should not be less than two-thirds of the total difference $p_1 - p_2$ of pressure on opposite sides of piston.

To illustrate further, let the pump be working where

$$\frac{p_1}{p_2} = 6, \text{ and } \tfrac{2}{3} \text{ of this gives } \frac{p}{p_2} = 4.$$

Then the table for eq. (14) gives

$$\frac{P}{p_2} = 2.5 \text{ for } \frac{p}{p_2} = 4\,;$$

and this, combined with eq. (9), gives

$$1.5\,p_2 = \frac{Ez_1^4}{48r^4}.$$

If $p_2 = 1$ atmosphere $= 15$ pounds per square inch, and if the ring be made of cast iron, as probably a good material, when $E = 20{,}000{,}000$. Then

$$\frac{z_1}{r} = .086.$$

If the pump has a 12 inch cylinder,

$$z_1 = .514 \text{ inch},$$

so that for the conditions assumed, a 12-inch piston packing ring for a pump will be about $\frac{1}{2}$ of an inch thick at the thickest place.

The eccentricity, a, for use in turning the ring, will be $a = .206\,z_1 = .12$ inch, or about $\frac{1}{10}$ of an inch.

These results are for a pump where the pressure, p_2, on the suction side of piston is one atmosphere; and p_1, on the pressure side, six atmospheres.

For similar conditions, except the working pressure, $p_1 = 2$ atmospheres, we have

$z_1 = .294$ inch, or about $\frac{3}{10}$ of an inch.
$a = .06$ inch.

It is noticeable, from eq. (9), that z_1 is proportional to r for a given pressure, so that the thickness of ring for other sizes than 12 inch pistons can readily be obtained.

If we ignore the refinements about the orifice of entry, and assume p as identical with p_1, we only require formulas (14) and (9) to determine the ring. In fact, they may be combined, giving

$$\left(\frac{z_1}{r}\right)^4 = \frac{24p_2}{E}\left(\frac{p_1}{p_2} - 1\right) \quad . \quad . \quad . \quad . \quad (19)$$

Values of z_1 computed by this for the above examples give .588 and .392 respectively, in place of .514 and .294, as corrected for the orifice of entry.

These differences being comparatively unimportant, the effect of orifice of entry will be ignored in the following cases:

2. *Case of an Elastic Fluid like Air.*—Here the flow will be at some intermediate condition between the adiabatic and isothermal for the gas while escaping the packing ring. Considering it adiabatic, the proper expression taken from the article previously referred to is:

$$\frac{f\, s\, v_2^2}{a\; 2g} b = \frac{\gamma}{\gamma+1} \frac{p_2}{\delta_2} \left\{ \left(\frac{p_1}{p_2}\right)^{\frac{\gamma+1}{\gamma}} - 1 \right\}$$

where v_2 is the velocity of escape and p_2, and $\gamma = 1.408$ for a perfect gas. For the present case this eq. may be put under the following form:

$$\mathrm{A}b = \left(\frac{p_1}{p_2}\right)^{\frac{\gamma+1}{\gamma}} - 1 \quad . \quad . \quad . \quad . \qquad (20)$$

or for shorter distances, x, reckoned from the exit side of ring, where also the pressure will be p, we have

$$\mathrm{A}x = \left(\frac{p}{p_2}\right)^{\frac{\gamma+1}{\gamma}} - 1$$

or

$$(1 + \mathrm{A}x)^{\frac{\gamma}{\gamma+1}} = \frac{p}{p_2}$$

The value of p from this, introduced into eq. (13) and integrated, gives

$$\mathrm{P}b = \frac{p_2}{\mathrm{A}} \frac{\gamma+1}{2\gamma+1} \left((1+\mathrm{A}b)^{\frac{2\gamma+1}{\gamma+1}} - 1 \right).$$

Restoring the value of A by aid of (20) we obtain

$$\frac{\mathrm{P}}{p_2} = \frac{\gamma+1}{2\gamma+1} \left\{ \frac{\left(\frac{p_1}{p_2}\right)^{\frac{2\gamma+1}{\gamma}} - 1}{\left(\frac{p_1}{p_2}\right)^{\frac{\gamma+1}{\gamma}} - 1} \right\} \quad . \quad . \quad . \qquad (21)$$

an expression in terms of p_1 and p_2 only. Hence a table is readily computed for practical use, thus:

If

$\frac{p_1}{p_2} =$	$\frac{\mathrm{P}}{p_2} =$	
4	2.72	
6	3.94	(E)
8	5.18	
10	6.42	

By employing these values of P in (9), the thickness of ring, z_1, can be calculated.

If the flow past the ring be regarded as isothermal, γ, in equation (21), is to be taken equal 1, and hence

$$\frac{P}{p_2} = \tfrac{2}{3}\left\{\frac{\left(\frac{p_1}{p_2}\right)^3 - 1}{\left(\frac{p_1}{p_2}\right)^2 - 1}\right\} \quad . \quad . \quad . \quad . \quad . \quad (22)$$

and hence the following table:

If	$\frac{p_1}{p_2} = 4$	$\frac{P}{p_2} = 2.80$
	6	4.09
	8	5.41
	10	6.73

These values of P differ only slightly from those in (E), so that as regards the pack-ring question, since the 4th root of these quantities is to be taken in (9), the difference for the adiabatic and isothermal condition is entirely insignificant. Even compared with the table (A) for pump-packing rings, the 4th root will differ but slightly for a given ratio of pressures, p_1 and p_2.

Hence, the following table will be found to cover nearly the whole range of practice:

PRACTICAL TABLE FOR SINGLE RING PACKING.

			Pumps.	Engines, etc.
For	$\frac{p_1}{p_2} = 4$	$\frac{z_1}{r.p_2^{\frac{1}{4}}}$	= .0431	= .0451
	6		.0495	.0509
	8		.0538	.0560
	10		.0573	.0600
	12		.0603	.0634

p_1 = higher absolute pressure, pounds per square inch, on piston.

p_2 = lower absolute pressure, pounds per square inch, on piston.

r = radius of cylinder bore, in inches.

z_1 = thickest place in the packing ring, in inches.

Eccentricity of a ring $= a = z_1 \,.206$.

The ring is supposed to be turned so that when sprung into its groove $\frac{2}{3}$ of it will lie outside of the groove before the piston is put into the cylinder. This is realized by making the outside diameter of ring when turned $= d_1 = d + z_1$ nearly, where d = the diameter of cylinder bore. The inside diameter, d_2, at which to bore out the ring will be $2(r - z + a)$, or

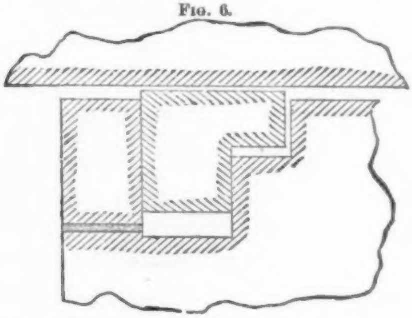
$$d_2 = d_1 - 1.588\, z_1$$
$$= d_1 - 1.6\, z_1 \text{ nearly.}$$

The groove for the ring is to be turned out on the solid face of the piston, the depth being somewhat greater than the thickest place, z_1, of ring.

The width of the groove should be such as to give an accurate free fit to the ring.

The width of ring is arbitrary, the above formulas (9), (14), (21), and (22), for determining the ring not containing any term for breadth. The total pressure of ring against the cylinder is proportional to the width for a given thickness. Hence, the pressure per square inch of surface is constant. The rate of wear of the ring from its surface will be proportional to the pressure per square inch. Hence, a wide ring will wear no longer than a narrow one. A wide ring will, however, wear the cylinder bore more than a narrow one, and also leave narrower rims on the

FIG. 6.



piston for wear. Hence, as a narrow ring is, according to the formulas, as efficient in preventing the escape of the working fluid as a wide one, it is decidedly best to adopt a narrow ring, usually not to exceed a half an inch.

In forming the lap shown in Fig. 3, a portion of the ring must be cut away. The demand for this is met in the ring being turned larger than the cylinder, thus requiring some to be cut from it.

According to directions above, the excess in diameter over the cylinder bore, at which the ring is turned, is z. If the ring did not elongate along its exterior surface as it is sprung into the cylinder, we would find the amount cut out $= \pi z_1$, since the outside lengths of the ring, before and after, would be $\pi d_1 = \pi (d + z_1)$ and πd, the difference of which is πz_1. But as the outside elongates slightly in the flexure, we find by considering the amount of flexure, position of the neutral axis, etc., that the amount to cut from the ring $= \pi z_1 \left(1 + .397\frac{z_1}{r}\right)$.

In regard to setting the ring out by pressure of working fluid beneath it, introduced through holes from the pressure side of piston, it is evident that the pressure of ring upon cylinder per square inch will be nearly $p_1 - p_2$, while the above figures make it $P - p_2$. The former pressure, as seen by tables (A), (E), and (F), is about fifty per cent. greater than the latter, showing that the so-called steam packing is to that extent too heavily set, causing undue wear, resistance, etc. The steam packing ring should, therefore, be made as in Fig. 6, with pressure under only a part of it, the wing being relieved to prevent receiving pressure.

When several rings are employed on one piston each might be so designed as to act for reducing only part of the pressure, and the rings could be made somewhat thinner than where one is depended upon for making the joint. The "practical table," however, indicates only a reduction in thickness of from a fourth to a third.

XXI.

*EXPERIMENTS WITH NON-CONDUCTORS OF HEAT.**

BY CHARLES E. EMERY, PH.D., NEW YORK CITY.

I WAS called on about a year ago to investigate the subject of supplying steam through underground pipes, in connection with a district system in New York city. I found that steam had actually been conducted to points a mile from the boiler. I did not find that there was very much accurate information as to the economy of the system. It was practicable, to a degree; that was certain. They were carrying steam as far as they sometimes carry gas, and people were comfortably warm. Steam for power was also carried a considerable distance. It became necessary, however, before going into it on the scale that would be required in a large city like New York, to thoroughly investigate each one of the different principles involved in construction, the flow of steam in pipes, but particularly this question of non-conducting materials; those which were non-combustible being of prior importance. For this purpose an apparatus was constructed on a much larger scale than anything I know of having been done before, which has given us some very excellent results. A boiler, something like four feet in diameter and twelve feet long, was made with three ten-inch tubes, with their ends open externally.

* A verbal communication to the Society of Mechanical Engineers, reported stenographically.

Into these tubes were placed smaller tubes to receive steam, and around the inner tubes were placed the non-conducting materials; water being circulated through the larger shell outside of the outer tubes. The results were shown by the amount of steam condensed in the inner tube, the water of condensation being conducted to small cylindrical vessels each provided with a glass gauge.

The line of pipe for supplying steam to these internal tubes was drained at various points, and finally the steam was admitted to a drum below the level, and the pipes that led to the interior of the tubes were carried vertically up at one end, and at the other end other pipes were carried down to the smaller vessels. The whole apparatus was swathed very thickly with felt; three sides of the glass gauge were covered permanently with felt, and a felt covering provided over the remaining side. The person observing turned up a little corner of the felt and looked in, so the effect of the different non-conducting materials placed in the pipe was accurately determined, as the small condensations outside due to the temperature of the air would not materially affect the results.

The first series of experiments was made with tubes of different sizes and varying thicknesses of material, to find the effect of different thicknesses of a constant material, wool or felt, or whatever it might be. The results of those experiments I cannot give now; I have not had time to write out a paper, and besides, it developed an apparent contradiction, a thing which we have to meet in our experiments sometimes. The fact was that the smaller tubes condensed more per square foot of surface than the larger ones, although they had more non-conducting material around them. That was true of wood as a non-conducting material. The reason of it becomes evident when we consider the radial lines on which the heat is transmitted from the pipe to the absorbent about it. The loss due to the increase of external surface increases faster than the gain due to additional thickness. A little mathematical calculation will, probably, bring all the experiments to the same basis; and at this time I will only attempt to show the general result of the other series of experiments in which the sizes of pipes were kept constant, and the thickness of the non-conducting materials kept constant, as such tests simply show the relative value of different kinds of non-conducting materials, and the results are very interesting. Hair felt was the best non-conducting material of all those tested, and the value of two inches is taken as unity

and the maximum. The value of two inches of mineral wool as a non-conductor was 0.832 of hair felt; two inches of mineral wool and tar was 0.715; two inches of sawdust, 0.68; two inches of a cheaper grade of mineral wool, 0.676; charcoal, 0.632; two inches of pine wood, across the grain, 0.553; two inches of loam, 0.55. This was from the Jersey flats, and almost all vegetable fibre, not yet become compact. Slacked lime, from the gas works, expressed decimally, with hair felt as unity, 0.48; coal ashes, 0.345; coke, only 0.277, the same as used for fuel; two inches of air-space only 0.136, which dashes a great many people's hopes, and is as interesting as any part of the data; two inches of asbestos, 0.363; two inches of Western coke, about the same as the other coke; two inches of gas-house charcoal, 0.47; the other charcoal being 0.63.

Now, these are very interesting, particularly so this matter of an air-space. It has been supposed that an air-space around a pipe is about as good as anything we can have. The fact is, convection or circulation takes place; the air is cooled on one side of the space, descends, and rises on the other, and it is necessary to break up the air-space, and that undoubtedly accounts for the efficiency of these different materials. It is the air, probably, which is the non-conductor; but it should be kept quiescent instead of being allowed to circulate. The air-space itself is of very little value until the circulation is prevented.

DISCUSSION.

MR. DURFEE: I would like to ask Mr. Emery if he has ever tried any experiments with an envelope constructed anything in this way, with an air-space, and then a surrounding envelope of hair, felt, or other material?

MR. EMERY: Yes; that was tried inside of wooden logs, and it was not as efficient as to fill it all up with wood. I could not give the whole list. I have simply taken what I had in my pocket for reference. I thought it would interest the gentlemen present. It would take quite an elaborate paper to go through all that we have been over. The variations in the results need to be discussed mathematically, or else they look contradictory. These being all on the same basis can be presented for comparison.

MR. FULLER: Mr. President, I desire to inquire if the conditions under which the water entered the cylinder were such that each of the tubes had an equal chance—if the temperature of the water was preserved equally round them all?

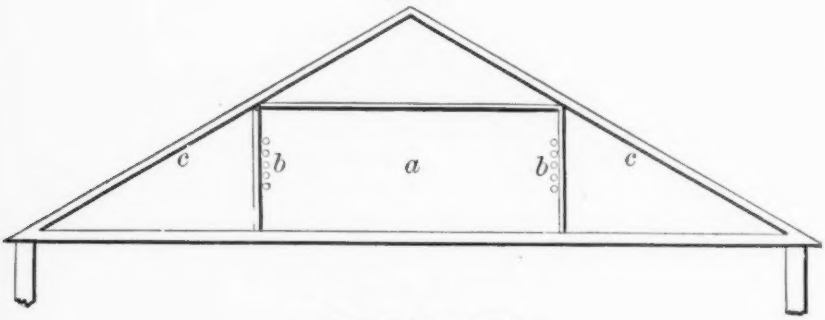
Mr. Emery: Yes, sir; and at first I made some changes in the position of the tubes, tried them over again to settle that very point; and for a long time the felt was kept in one of them, and the other two were filled with other materials. But the water was taken from the Croton, and there was such a small quantity of steam condensed that the difference between the initial and final temperatures was only two or three degrees, so that it was impossible for a result to be very different in any two cases; besides, I changed them about without noticing any material difference between the experiments. The results given are not from single experiments, but averages of a large number.

Mr. Durfee: I have never had any opportunity to make any extended experiments in the matter of covering pipes. But on one occasion I had to have covered quite a long length of 14" pipe and it was covered in this way: there were some rings about an inch and a half thick, put at intervals around the pipe; involving those rings was wire cloth, and outside of that an inch and a half of hair felt; that again was enclosed in a thin covering of rubber cloth. I do not know what is the value of rubber cloth; I do not know what the value of that would be relative to any others; but it was very effective in the particular case I applied it to. On one occasion, during a very severe winter, when the thermometer was 30° Fahrenheit outside of the buildings, I noticed an icicle two feet long hanging on the inside of this covering.

Mr. Woodbury: In the way of solving this same problem of protecting these pipes from radiation, about ten years ago I had occasion to make some investigation on this point; and my results, as far as my memory serves me, are very similar to those of Mr. Emery. But the matter was finally obtained by hot water rather than by steam. I used pieces of iron pipe because I wanted to protect iron pipe, and therefore get as near the actual condition of the practice in that case as possible. Lengths of iron pipe $1\frac{1}{2}$" in diameter and about 14" long were set upright on a wooden base, and covered with the protector that I desired to test; there was a cork in the top and a thermometer running down to the centre of the tube, and a very small rubber tube also running to the bottom, and steam was blown in and the water in there was kept boiling for about an hour, in order that the specific heat of the iron and of the non-conductor should not be a disturbing factor. I wanted the whole matter to be uniformly heated, and then the steam was shut off, the water being at the boiling-point and the rate of its cooling

noted and recorded graphically. A short time ago, Mr. Hiram Kilburn, the agent of the Potomska Mills, in New Bedford, gave a receipt for a material for covering, which has proved very satisfactory in actual practice, although I cannot give any observations as to its non-conducting power. But I think it may be of some use to you and so will give it. It has the first advantage of being cheap. This is the receipt: Four parts of coal ashes sifted through a quarter-inch-mesh sieve; one part plaster of Paris; one part sour flour; hair, same as for mortar for inside work; wet the ashes and put in the hair; then work up to a regular mortar; mix the plaster and flour, and then mix all together; put on with a trowel; finish the outside with a mixture of plaster of Paris, lime, and sand, the same as for a hard finish. It adheres quite closely, and next to the pipe is porous. The sour flour in its fermentation

FIG. 7.



SECTION OF FACTORY ROOF.

generates considerable gas, and the covering is very porous; and the hard finish outside protects it. It is desirable that the pipe should be warmed with steam when the mixture is applied.

In regard to the convection of air I have a little example in mind. The Hampden Cotton Mills, in Holyoke, was a mill of the olden style, with a square pitched roof and sealed spaces at the sides and top—shown at *a* in the engraving—rendering the room very nearly square. The insurance companies frown on all concealed spaces. At *b b* appear the steam heating pipes in section. That room was so cold that it was uncomfortable. In winter it was practically impossible to keep that room warm. But, by reason of insurance interests those partitions were removed, and the wood of which those partitions were composed was used for sheathing the roof on the inside. The roof in the first place was made of 1 inch

plank covered with slate. The sheathing was screwed up underneath the roof *c c* (of course it would break the slate if they had used nails), increasing the thickness about seven-eighths of an inch; the same steam pipes were left there, and that room last winter was so warm that the help worked in there without inconvenience. The cause of the difference is that there is a freer circulation of air in the room, which enables the whole room to be heated; which is, I think, confirmatory of the statement made by Mr. Emery relative to the poor effect of large air-spaces as a non-conductor.

MR. EMERY: These experiments were made to obviate an imperfection in the method which Mr. Woodbury stated. He informs us that the experiments were made with hot water, letting it remain until it cooled down a certain number of degrees. Such experiments might be confirmatory, but they could not be accurate, for the reason that the specific heat of the surrounding materials is imparted to the water within to a certain extent, and thereby affects the result. The result is really an average for the two—the specific heat of the water and the specific heat of the material surrounding it; the material giving away the heat, and helping to impart its heat to that of the water. The other method, having a constant temperature inside, and a constant temperature outside, put the pipe in the condition in which it would be used in the streets, and the temperatures from one to the other after running a little while, are regularly graded, and correspond exactly to what would be obtained in practice.

MR. STIRLING: I would like to ask Mr. Emery if he experimented with coal-dust or fine anthracite coal?

MR. EMERY: Coke holding so much more air, and charcoal giving such poor results, it was not thought desirable to take anything so solid as anthracite; but there were some particles in the ashes that were experimented with that could not be removed.

PROFESSOR EGLESTON: I would like to ask Mr. Emery if his wool was disintegrated or slacked, and also if it was packed very much?

MR. EMERY: Not packed much; it was put in with as little pressure as possible. We have not made any experiments ourselves in regard to disintegration, or anything of that kind, except this. By dropping the wool into water, and letting it remain a time, it will break up quite readily. It may properly, I think, be called disintegration; it is a mechanical matter, not a chemical change.

PROFESSOR EGLESTON: As the slag will not be of constant

chemical composition you will have a blast-furnace wool of variable quality. If the process referred to was slacking, the metallurgists would be able to know what the matter was. If it is disintegration, why, that is a physical defect, and I think that could be remedied too.

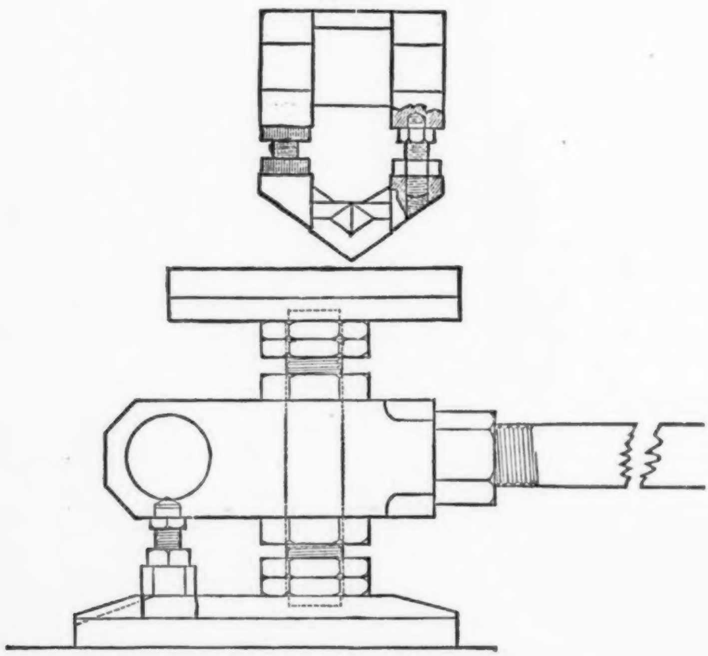
XXII.

ALTERATION TO THE CROSS-HEAD OF A CORLISS ENGINE.

BY CHARLES E. EMERY, PH.D., NEW YORK.

It will be remembered by the gentlemen present—in reference to engines with side frames, carrying guides, usually of "V" shape, above and below the piston-rod, as introduced years ago by Mr. Allen, of the Novelty Iron Works, New York, and made popular by

Fig. 8.



ALTERATIONS TO CROSS-HEAD OF CORLISS ENGINE.

Mr. Corliss in his engines—that the customary form of cross-head is, in general terms, a spade handle, provided at one end with a fixed pin for receiving one end of the connecting-rod, and at the

other with threads to receive the piston-rod, the connection to each slide, or gib, being through a single large set-screw arranged vertically in the body of the cross-head, behind the pin, and at some distance therefrom, so as to give ample room for the removal of the connecting-rod strap and brasses. That is, the vertical thrust of the connecting-rod is not carried through the slides to the guides, directly under and above the cross-head pin, as it should be, but at a point so far behind the cross-head pin as to practically make the cross-head and piston-rod a lever, with the set-screws as a fulcrum, operating to lift the piston up and down in the cylinder and produce unequal wear, and frequently noise, when the engine is heavily loaded.

The cause of the ill-working of pistons in Corliss engines in some cases was not generally understood, and gave rise to many changes of detail in the piston itself. Some suggestions were made by those who had ascertained the true causes of the difficulty, but it was not till a comparatively recent period that Mr. Corliss changed the form of his cross-head for his larger engines, to bring the bearing of the slides directly under the pin.

The writer recently had occasion to put in a second-hand Corliss engine, with a cylinder 16 inches in diameter and 42 inches stroke of piston, which had a cross-head of the kind first above referred to, and considered the defect of sufficient importance to warrant the change in the construction of the lower gib, also shown in the engraving.

As will be seen, the bottom gib is simply extended towards the crank, some little distance beyond the centre of the cross-head pin, and small set-screws placed either side, under the jaws of the spade handle, to transfer the vertical thrust directly to side ears on the gib. The rear set-screw and jam nut are made with notched cylindrical edges, so that they may be adjusted with a set or chisel.

Other details will be readily understood from the drawing.

The arrangement operates well.

Of course, the additional length of gib runs off the end of the guide, which is no objection, and in fact is utilized by placing there a small box, with pad, to lubricate the gib each time it reaches the end of the stroke.

DISCUSSION.

Mr. Leavitt: Mr. President, I would like to say a word on this subject. I think Mr. Emery has made an improvement on Mr. Corliss's cross-head. This cross-head has caused many very serious

accidents. With such a one at the Merrimack Mills, at Lowell, the piston-rod broke and smashed up the engine. The fracture was almost perfectly smooth, as though the surfaces had been rubbed together. The rod was five inches in diameter.

Mr. Strong: I would like to ask the gentleman if the action upward on the outward thrust would not be the same as it is downward?

Mr. Emery: The gentleman probably will recollect that the strain on the guides is always one way, so long as the engine is run in the same direction. In this case, during one stroke, the rod pulls at an angle, and causes pressure on the bottom guide, and during the return stroke pushes at another angle, so as to cause pressure on the same guide.

Mr. Strong: That may be in the case where the fly-wheels never carry the load. But in the case of rolling-mill engines, where the fly-wheel carries the load—generally more than the piston does while the work is being done—would not the action then be the other way?

Mr. Emery: There would be some action, but the rigidity of the parts would probably take it up. It would, probably, be better to put set-screws on both, but on a light-running engine, if it will work at all without something of that kind, this will entirely overcome the difficulty in the direction in which the main part of the load acts. It was a mere makeshift that I thought I would call attention to here, as it is so simply fixed without reconstructing anything except the gib.

XXIII.

USE OF THE CALORIMETER AS A PYROMETER FOR HIGH TEMPERATURES.

BY J. C. HOADLEY, C.E., BOSTON, MASS.

Having undertaken to prepare and conduct some experiments on the combustion of coal in the furnaces of steam-boilers, with warm blast, some instrument for determining the temperature of the products of combustion with satisfactory accuracy and convenience seemed a desideratum. The Siemens pyrometer, or specially modified calorimeter, briefly described in D. K. Clark's Manual, appeared to promise the most satisfactory results; but

after considerable inquiry, I could not find one in this country, and, therefore, set about designing and constructing one. Several questions at once arose:

1. The most advantageous quantity of water to deal with.

2. The mode of constructing the vessel so to diminish the transmission of heat as to eliminate, as far as practicable, the disturbing influence of external temperatures.

3. The best materials for the vessel.

4. The substances to be selected for conveying a specific quantity of the heat of the furnace to the water in the vessel.

5. The form, dimensions, weight, and specific heat of the substance selected.

6. The method of heating such heat conveyer, and of transporting it with least loss of heat to the water.

7. Disposition and arrangement of thermometers.

8. Details of manipulation.

First, the quantity of water to be used. Clark says that Siemens's vessel contains about a pint, and that his scale is one-fiftieth; that is, a rise of 1° in the temperature of the water indicates a fall of 50° in the temperature of the heat conveyer. In view of the high temperatures I expected to deal with, this scale seemed to involve an inconvenient increase of the temperature of the water. In effect, if the temperature of the furnace were 2000° F. above the initial temperature of the water, the water must be heated 40°, which, even if divided, part below and part above the temperature of the room, would involve an inconvenient rate of heat transmission. Besides, the calorific capacity of the vessel would, in so small a vessel, bear so large a proportion to that of a single pound of water as to introduce a needless amount of uncertainty. For these reasons, and because a larger vessel would better admit of applying all needful or desirable thermometers, without becoming unwieldy, I fixed on two pounds of water, including with it the calorific capacity of the vessel, agitator and appurtenances, so far as these cannot be insulated, but must share to the full and almost instantly all changes of temperature in the water. The calorific capacity of a suitable vessel I found by calculation, confirmed by experiment, to be about 0.0757 pounds of water; requiring, therefore, 1.9243 pounds of water; making the whole 2.0000 pounds of water.

The several vessels may differ a little among themselves, and require each a separate determination. The water required is

conveniently measured in a measuring bottle, with straight, graduated neck.*

For materials, I have used tentatively, tin plate, copper, and brass. But for danger of corrosion, tinned iron would be most available, as it could be used very thin, and its heat capacity could be kept low.

But as the vessel is necessarily rather costly, it is safer and cheaper in the long run to use something more durable. Silver would be excellent, but, perhaps, no better than copper. Both, however, are so soft that considerable thickness is required to obtain sufficient firmness. On that account, I finally selected sheet brass. I find only a single determination of the specific heat of brass, while of copper there are many; but since the calorific capacity has, after all, to be determined by direct experiment, this consideration is of no great importance. Sheet brass, .01 inches thick, is sufficiently firm.

For the twofold purpose of preventing corrosion and impeding radiation, all surfaces are nickel-plated and burnished.

Silver would, on some accounts, be a little better, perhaps, than nickel; but its use in combination with vulcanized rubber is inadmissible, on account of the action of sulphur on silver.

The cup proper is insulated from the rest of the vessel by a considerable thickness of hard rubber, and the several shells of the case are insulated from each other in the same manner. The handle of the agitator, and the rim and central tube of the cover, are of the same material. The case is about 1.75 inches thick, composed of three concentric cups around the inside cup, and divided, therefore, into three concentric chambers, with walls of burnished nickel. The same construction extends to the cover. These chambers are all filled with eider-down, just sufficiently compressed to completely fill the space, for the double purpose of intercepting radiant heat and preventing convection, by impeding circulation of air. A small tube is provided in each space, for inserting a thermometer. These tubes are three-eighths of an inch in diameter, and flaring at the upper end, to receive a cork. The tubes are coated with lampblack on the outside, and are set in the middle of their respective chambers, but without touching either wall. Similar tubes, but much shorter, are provided in the compartments of the cover. The thermometers are of use in observations for determining by experiments with hot water, the

* It is better to weigh the water.—J. C. H.

real calorific value, referred to water, of the vessel and its appurtenances, in so far as these are sensibly affected by internal temperatures, during the period of time occupied by a pyrometrical observation.

These test experiments were conducted in the following manner:

All compartments, and all parts of the vessel, having been brought to the temperature of the room, which was maintained as steadily as possible at 69° F., a known quantity of water, weighing nearly two pounds, was put into a copper pipkin, and heated to about 180°, when it was taken from the fire, and the thermometer, which showed a gradual reduction of temperature, was carefully watched. All things being in readiness—time observed—just before the thermometer showed 178° the water was quickly poured into the cup, and the cover was closed; the agitator was actively plied, and the thermometer was narrowly watched. In half a minute from beginning to pour, the mercury had reached its highest point, 175°, and began to recede—slowly at first, and then rapidly. In about one minute some attention could be spared for the thermometer in the space nearest to the cell, or cup, which began to feel the influence of heat from the hot water soon after the first minute. For a few minutes observations had to be taken as rapidly as possible, but changes soon became less rapid, and observations of the seven thermometers, one in the cup, and one in each of the three compartments of vessel and cover, could be taken regularly, seriatim, in a constant order. For half an hour this was done, as nearly as possible once a minute, then once in two minutes, then once in six minutes. The temperature of the room, which changed but little, was noted at intervals. After four hours, the changes became so regular and so moderate in amount that observations at longer intervals sufficed to define the curves of temperatures, and the vessel was set into a closet where the temperature was very uniform, and, after a few readings at intervals of fifteen or twenty minutes, left for the night. Occasional readings of the thermometers during the night, the following day, and the succeeding night, traced the respective curves of temperature far on towards their slow equalization.

Changes in the temperature of the room due to open windows and an extinct fire at night, and a rekindled fire and closed windows in the morning, reflected by corresponding, but lesser changes in the closet—and by changes in the rate of cooling—progressively less and less marked, but still corresponding in

time and direction with changes in the room, were noted in all the compartments and in the central cup. All these several curves being carefully plotted on a diagram, I next made a diagram of the loss of temperature in the central cup, referred to the synchronous temperatures in the middle compartment as a base line. Dividing this curve, which was very symmetrical and regular, into intervals of 5°, I ascertained the interval of time corresponding to each 5°, at the respective differences of temperature between the central cup and the middle compartment, and computed the loss per minute, and the time per degree. These differences and rates of transmission tabulated, gave me corrections to apply for any length of time occupied by a pyrometer observation, at any observed difference of temperature. The experiment also gave the calorific capacity of my cup and appurtenances, equal, say, to .0757 pounds of water. Taking, then, 1.9243 pounds of water, the whole vessel and contents were equal to two pounds of water. I subjoin, in a tabulated form, the ascertained rates of heat transmission.

TABLE.

Differences of temperature.		Interval of time per five degrees.	Time per degree.	Degrees per minute.
Greatest. Degrees.	Least. Degrees.	Minutes.	Minutes.	
55	50	10	2.	0.500
50	45	19	3.8	0.263
45	40	28	5.6	0.179
40	35	33	6.6	0.152
35	30	40	8.	0.125
30	25	51	10.2	0.098
25	20	75	15.	0.067
20	15	114	22.8	0.044
15	10	180	36.	0.028
10	5	324	64.8	0.015
5	0	500	100.	0.010

We come now to a consideration of the substance to be selected as a heat conveyer from the furnace to the calorimeter. For the highest obtainable temperatures the choice seems, indeed, almost limited to platinum, that metal, with all its inconveniences, alone possessing a melting-point sufficiently elevated, and a specific heat pretty satisfactorily determined. The principal objections to its use are its rather high cost and its very low specific heat.* It now costs, in rods or sheets, $8 per ounce, troy, equal to $96 per pound, troy, or $116.67 per pound, avoirdupois. This is one and two-thirds cents per grain, or three grains for five cents, or

*** Further experience has convinced me that only platinum can be relied on. The specific heat of iron is too variable at varying temperatures.—J. C. H.**

sixty grains for a dollar. As scrap metal, it sells at $5 per ounce — a discount of thirty-seven and one-half per cent. The specific heat of this metal is not so readily ascertained. I find in Clark's tables — *Constants of Nature*, published by the Smithsonian Institution, No. 276 — thirty-five determinations of the specific heat of platinum, at temperatures ranging from 0 to 950° C. (32° to 1742° F.). While the discrepancies in these determinations are not very great, they are just sufficient to make a selection difficult. Each observer, who made several determinations at various temperatures, finds a slight increase of specific heat for the higher temperatures; but many of the determinations at the highest temperatures given are lower than others at ordinary temperatures or below, down to the freezing-point. The names of very celebrated chemists carry a certain weight; but even the great name of Regnault cannot outweigh the numerous results reached by other careful experiments. A careful perusal and study of all the original memoirs might reveal some cause for giving decided preference to one or another set of determinations; but these memoirs were not accessible to me, and time for so laborious an investigation could not be spared. I therefore plotted all the results, thirty-five in number, on a vertical scale of one thousand inches to unity — or the specific heat of water of maximum density—so that the fifth place of decimals was legibly expressed, and drew a curve such as should, in my judgment, best represent the probable mean. This curve was a straight line, sensibly parallel to the base; that is, the specific heat of platinum appears to be constant at all temperatures up to about 2000° F.—say 1093° C.—and to be 0.03333, or equal to one-thirtieth of that of water. As this metal, even at these temperatures, is still far below its melting-point, which is said by practical workers in it to be at or above 4000° F.—say 2204° C.—it seems not improbable that its specific heat is, at temperatures under 2000° F., sensibly constant, and not widely different at even 2500° F., or 1371° C.

This valuable quality, which is shared in an equal degree, so far as known, by no other substance, renders platinum indispensable for determining the thermo-convectivity of other substances. But for actual use, the high cost and low heat capacity of platinum almost unfit it.* After a careful consideration of all available substances, I selected wrought iron as best suited to my purpose. But

* Its heat capacity per unit of volume is not inconsiderable, on account of its high specific gravity.—J. C. H.

wrought iron, at the high temperatures I expect to encounter, would be so nearly fused as to be soft, plastic, sticky, and difficult to handle. It would also oxidize so rapidly as to require very frequent renewals, making it, work on it taken into account, little less costly than platinum. I therefore resolved to procure wrought-iron balls, coated with a firm capsule of platinum. The size and weight fixed on were:

Iron, . . .	0.88″ diameter,	weighing 700 grains.
Platinum, . .	0.98″ diameter, 0.05″ thick,	700 grains.
	Total weight of Fe + Pt =	1400 grains.

The specific heat of wrought iron was arrived at in the same manner as was that of platinum; but less satisfactorily on account of the close proximity of its melting-point, and the consequent rapid rise of its specific heat. As nearly as I could fix it, the specific heat of wrought iron at temperatures in the vicinity of 2000° to 2500° F. (1093° to 1371° C.) is about 0.166, say one-sixth that of water, and five times that of platinum. There can be little doubt that it is even higher at the highest temperature above mentioned; and if actually melted, its latent heat would be an unknown quantity, probably quite large.

I, therefore, propose to determine the actual thermic value of my composite balls (Fe and Pt), by actual comparison with platinum, heated in the same crucible, exposed to the same fire, and cooled in calorimeters exactly alike in all respects, as far as it is possible to make them so. By exchanging these vessels, and making numerous comparisons, it seems to me possible to reach pretty trustworthy results as to the specific heat of wrought iron up to its melting-point, and beyond.

For a first approximation, we have:

0.1 pounds (avoirdupois) Fe of specific heat	= 0.166,	= say .0166
0.1 pounds Pt of specific heat . . .	= 0.033,	= say .0033
Combined calorific value =		.0200

which is 0.01 of the 2.0 pounds of water, including the equivalent of the metal of the vessel we are dealing with.

Our scale will therefore be 100° to 1°, *i.e.*, each degree gained by the water and vessel will be balanced by 100° lost by the composite ball. By using two such balls the scale becomes 50° to 1°. If the actual specific heat of the wrought iron is found to require correction, as is not unlikely, a corresponding correction can be made in the quantity of water—preserving the relations of 100° to 1° for a single ball, and 50° to 1° for two balls.

The method fixed on for heating and transporting these balls, is as follows:

A number of black-lead crucibles were procured, of the size known as No. 1, being 3″ deep, 2″ diameter at top, and about 1½″ diameter at bottom inside; provided with good covers, having lugs on top for convenience of removal.

Into one of these crucibles, one, two, or four balls can be put; say one composite and one platinum, or two composite and two platinum, and all heated together. A fire-brick, having a cavity formed in its upper side to receive a crucible, lies on the bridge wall, and is kept constantly at about the temperature of the fire at that point. An iron band embracing this fire-brick, with a handle for pulling it out and pushing it in, lies on the bridge wall, its handle extending through a small door on the side of the boiler-setting opposite the bridge wall; or a hole may be drilled in the brick near the end, into which a bent poker may be inserted to withdraw and replace it—and, of course, the crucible and its contents along with it.

A shelf at the back end of the boiler, on a level with or above the bridge wall, with a corresponding small door, hollowed fire-brick, and handle, affords equal facility for taking temperatures just before the gases enter the flues simultaneously with those taken at the bridge wall.

The crucible and its cover serve to diminish, in a notable degree, the loss of heat during transfer of the hot balls from the fire to the vessel; and this loss, if sensible, can be closely approximated to by observation and experiment.

THERMOMETERS.

Influenced, perhaps, by prejudice, but not, methinks, without reason, I fixed on the Fahrenheit scale as, on the whole, the most convenient.

The instruments selected are, for the central vessel and the surrounding compartments, twelve inches long, graduated from 20° to 120°, which gives one-eighth inch to a degree, and admits of easy estimation of tenths of a degree.

The thermometers for use in the compartments of the cover are six inches long, similarly graduated, but, of course, with only about half as much space to each degree.

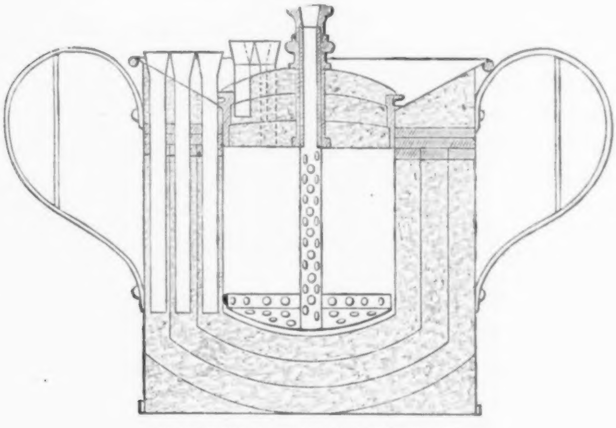
All these thermometers should be very carefully tested, both before and after using, and their errors noted.

They are, however, used simply as mercurial thermometers, no attempt being made to reduce their indications to those of an air thermometer.

EXPLANATION OF DIAGRAM.

I will now refer briefly to the diagrams shown herewith. The first is a sectional view of the pyrometer vessel on a scale of a little less than one-fourth. The cup or central cell is 4¼″ in diameter and 4″ deep. The bottom is made a spherical segment, for the double purpose of strengthening it, it being so thin, and giving the agitator such a form that the motion up and down should mix the water thoroughly. The spaces are filled with eider-down. There is a false bottom of brass inside of the outer case. These are all

FIG. 9.



SECTIONAL VIEW OF PYROMETER.

plated on both sides and burnished. They are attached by the contrivance, also shown, to a hard rubber ring, separating the flanges of the upper and lower portions of this central cell. A wide ring at the top serves as a clamp for the upper flange. A ring in each of these spaces, secured by screws, holds the whole firmly together, and all these spaces are filled with eider-down. In each space there is a tube for inserting a thermometer through a cork, and in each space of the cover; and in the central stem of the agitator the principal thermometer is inserted. The method of using is, after agitating the water and getting the temperature accurately, to raise up the agitator nearly to the cover, leaving sufficient space for the insertion of the ball; then by lifting both handles together,

until the top of the agitator comes to the inclined surface, when the metal from the crucible can be poured in and dropped into the water; and in dropping into the water the cover is closed, and the thermometer almost instantaneously reaches the maximum; which must be carefully looked to, for soon after it reaches the maximum

Fig. 10.

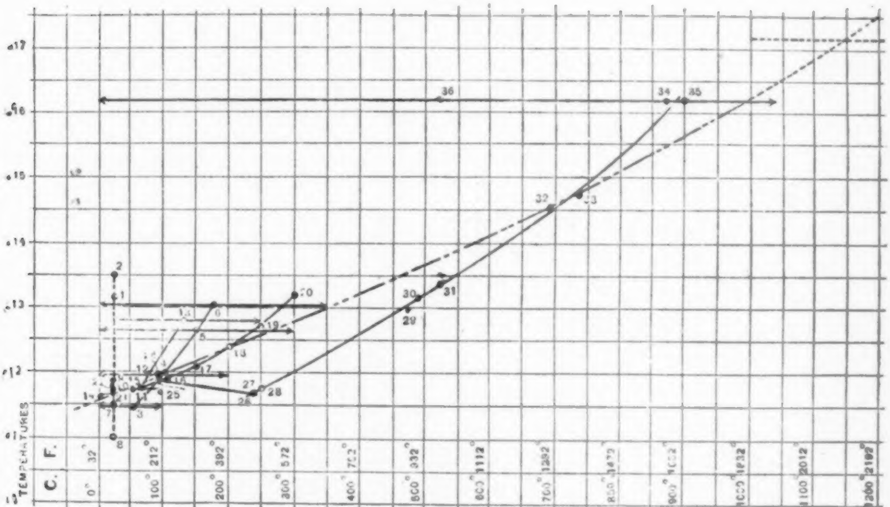
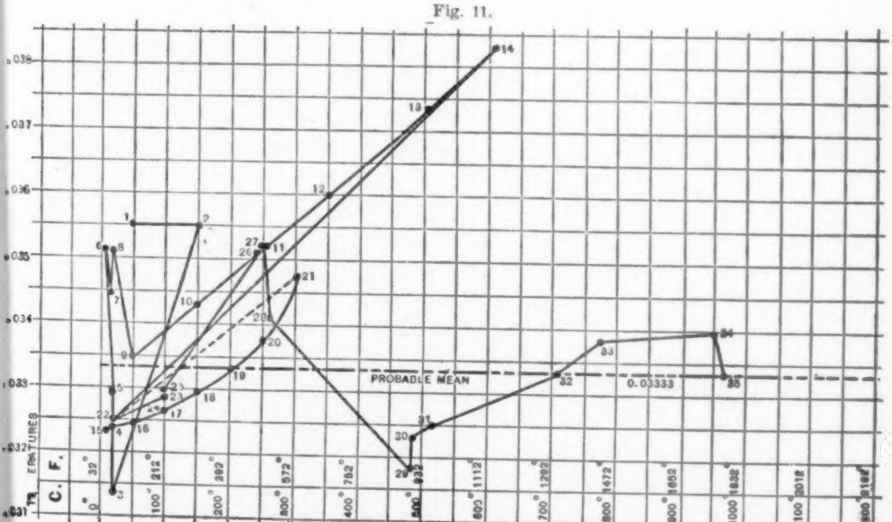


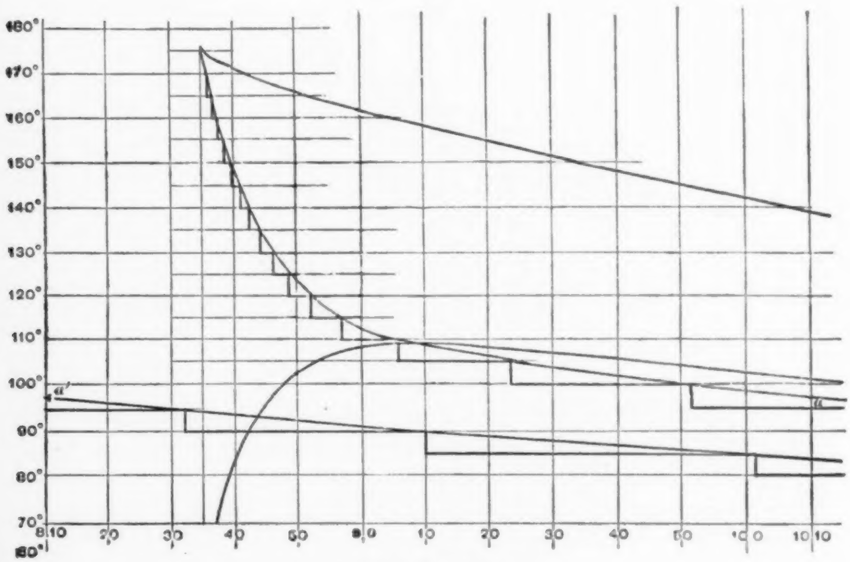
Fig. 11.



HEAT DIAGRAMS.

height it begins to recede. That being noted, we believe we have now the value of two pounds of water contained in the vessel, including the vessel itself below the insulation, the stem of the agitator, below the cover, and the agitator itself—and we know the weight of the ball and its specific heat—a simple calculation gives us the heat at which the ball was when it was inserted in the water. The lower plate of the cover is insulated, as you see. It has a little collar on top with a thread in it, and a hard rubber stem screws into it, and a hard rubber ring insulates it on the outside. These plates are put in to make compartments. The

Fig. 12.



HEAT DIAGRAM.

object of these compartments is to serve as a sort of wicket-gate to note the heat on its escape. I cannot conceive that they accelerate the passage of heat in any degree. They occupy but little space—a hundredth of an inch. They, therefore, take out but little eider-down. There is almost as much down as there would be if they were not there. But, as the specific heat of eider-down is not known, it is impossible to say how much heat it receives. An estimate of the quantity of heat which these surfaces receive can be made, and so of the others; and in that way I think that a very good check can be made upon the heat which necessarily escapes, because it is impossible to confine heat entirely. Then

these diagrams:—this first one was made originally on a scale of a hundred inches to unity. They are about double the size now —much reduced in the accompanying cuts, Figures 10, 11 and 12, —so they are about two hundred inches above the base line. The figures are decimals of the specific heat of water, .10, .11, .12, .13, .14, .15, .16. A great many determinations have been made at the ordinary temperatures, and there is a series here of very interesting determinations, which shows a gradual rise in the temperature. Still it seems to me impossible to admit that these specific heats can be right, weighing against them the great number of determinations at lower temperatures; and for a first approximation (of course it is a matter of judgment, and every man will be at liberty to choose his own), the dotted line, it seems to me; will probably be about correct, and at the temperatures that I expect to deal with, the specific heat will be something like one-sixth that of water. The diagram of the specific heat of platinum is on ten times as great a scale, drawn originally to one thousand inches. It is now two thousand inches, which would be one hundred and sixty-six feet to the base line—much reduced in the cut—consequently, these variations which appear considerable here, are very inconsiderable in comparison with the whole. But although Regnault's determinations show this ascending scale and other determinations an ascending scale, yet in view of the location of those several determinations upon the diagram, it seems impossible to doubt that the specific heat of platinum is somewhere about as I have placed the dotted line, which is a mean of the whole, and that is one-thirtieth that of water.

DISCUSSION.

THE PRESIDENT: I suppose it is known by most of the gentlemen who have done work of this sort, that this method of determining temperatures seems to be the only practicable one for very high temperatures. The pyrometers we purchase are of no value when they get above one thousand degrees; and at temperatures exceeding, say one thousand degrees, I presume, we must probably for a long time, if not for all time, use the method of which this is an illustration. I think Mr. Hoadley's ingenuity in getting up his calorimeter, and particularly his beautifully ingenious use of the combination of the two metals, is very interesting, and will probably prove very valuable. I am sure the members will be very glad to get some of these determinations.

And speaking of the Siemens pyrometer—about a year ago some work was done by some of our young gentlemen with the other form of Siemens pyrometer—the electrical pyrometer—that was quite interesting. A Ryder air-engine was tested by the use of the electric pyrometer combined with the use of the indicator. Well, the Ryder engine is an engine in which air is bottled up; put an indicator in it and the engine itself becomes a thermometer; using the Siemens pyrometer in conjunction with the indicator they made quite a series of experiments. They had great difficulty at first in stretching the wire so that it should not be affected by the action of the engine. They made a number of attempts before they got a wire that would stand. They finally did succeed in stretching a very fine wire, one that was, if I remember right, about 0.02″ in diameter, or perhaps less; and standardizing the pyrometer and using a very good set of resistance measures, galvanometers, they determined the resistance; used indicators to determine varying pressures and compared results of a determination of work done by each method for comparison; and there was a difference of but two, or three, or four foot pounds in the two sets of determinations.

Mr. Hoadley: I have heard of the use of the conductivity of platinum, the increased resistance of the conduction due to increased temperature; but have never had any experience with it, and have never been able to learn anything about it so as to make it practically available; and this seemed to be the only thing open to my use in this case.

Mr. Smith: I would like to ask the gentleman the temperature usually found in those Siemens furnaces.

Mr. Hoadley: I really cannot tell.

Mr. Holley: Dr. Siemens says they reach the heat of dissociation; he says that you can see the gases going through.

The President: You can see the gases dissociated?

Mr. Holley: Dr. Siemens says that he and other persons have seen those currents going through and not coming into combustion. They are more transparent before they come into combustion when the furnace was at its highest temperature, and gives that as an evidence that the heat of dissociation is sometimes reached.

Mr. Hoadley: These are my pellets (*exhibiting*), these three are platinum. The largest one is 0.6 of a pound, 4200 grains. This is a composite ball of 700 grains of iron encased in 700 grains of platinum. Its heat-capacity is equal exactly, as I suppose, to

the larger and much heavier ball of platinum. These balls have also, it is supposed, the same heat-capacity. They are intended for use at a lower temperature at the back end of the boiler, about the temperature of incandescence. They are wrought iron, simply coated with nickel, burnished, attempting thereby to prevent oxidation. I do not know how they will work. The copper balls are intended to be used in the same manner, and also to be nickel-plated, and they are curiously alike, notwithstanding the difference in specific gravity. The difference in specific heat is always exactly inversely as the difference in specific gravity; so that there is only about five or six thousandths of an inch difference in the diameter between two balls, having, I suppose, about the same heat-carrying capacity, the one iron, the other copper.

THE PRESIDENT: I presume, Mr. Smith, the temperatures Mr. Hoadley will measure with these are the ordinary boiler temperatures. These will run about 2000° Fah.; sometimes a good deal lower.

XXIV.

EXPERIMENTAL MECHANICS.

BY OBERLIN SMITH, BRIDGETON, N. J.

THERE is in this country a field of mechanical work, which is of vast importance to its industrial interests, and even to pure science, but which has never been occupied in any systematic way. I refer to experimental mechanics,—the ascertaining by tentative methods the fitness, strengths, and qualities of different materials, and their behavior under various strains, motions, processes, and continued uses; of their best forms and proportions when worked into parts of machines, and like considerations.

This work has, so far, been chiefly done by individuals, as they felt its absolute need in inventing and developing various machines. Some of it has been done by the National Government, principally to meet its own necessities in naval matters; a little, in the way of testing boilers, etc., to enable it to enforce its steamboat laws. Other portions of the field have been occupied by solitary scientific students and by learned societies, colleges, and technical schools, *e. g.*, the Stevens Institute, with its valuable tests of strength and elasticity of metals.

In France there is, I believe, some work of this kind done at government expense, but I have forgotten to just what extent; probably less since she has become a republic than when under the "one man power" régime. In this country we can hardly hope that our government will, in our time, be sufficiently under scientific influence, or alive to the magnificent industrial economy of the expenditure, to devote a few millions to the endowment of a great National University of Experimental Science, with its corps of well-paid professors, selected from the ablest talent of the world, and its thousand earnest students, all at work making records which would speedily be recognized as standards of technical practice.

In default of this the work must be done, as heretofore, by our chemists, and engineers, and mechanics, and electricians. It may be, however, that the time has come for the introduction of more method and system, in order that efforts which are now wasted in needless duplication may be devoted to more accurate *finishing* and *recording* of experiments, and making them accessible to the mechanical public in a properly indexed form. Incomplete experiments are the rule, rather than the exception, when performed by individuals in furtherance of some industrial result. This is simply because the required time and expense deter them from going any further than is absolutely necessary for the case in hand.

Apropos to this part of the subject, I have, in common with others, experienced on numerous occasions the want of a little systematized and "get-at-able" knowledge about some very simple matters. I have, however, always been obliged to fall back upon private experiments, which, in the nature of the case, would have been too expensive if made thorough enough to be of public value.

To select a few instances: Case "A" was regarding common spiral springs—the principles governing their action; the pressure to be obtained with a given motion, with given material, given diameter of coil and wire, and given pitch and number of coils. Nobody knew.

Case "B" was in relation to "drawing" sheet metals, where a flat disk of tin-plate, brass, or other thin metal, is drawn cold into a cylindrical or conical form. Who knows the sizes of these disks to form a given depth and diameter of pan or box? Only those manufacturers who have accumulated hundreds of samples, finding the disk sizes by actual trial (involving ofttimes tiresome alterations to expensive dies) from which they can guess approximately the dimensions for new patterns which they may wish to make.

Case "C" related to permanent magnets. How short could they be in proportion to their thickness? What attractive power had they in proportion to their weight, when magnetized to saturation? What time was required for such saturation with a given hardness of steel, and a given strength of electrical current in a surrounding helix? Would very minute magnets (say grains of steel dust) behave proportionally as larger ones, etc.?

Case "D" was the simple question: How fast is it safe to run an ordinary grindstone, and what is its bursting speed? A letter to a prominent grindstone manufacturer elicited the reply, that he did not know, but that Messrs. So & So ran their stones so fast, and found it about right. In regard to Case A, I wrote to gentlemen, eminent for scientific research concerning the elasticity of metals, and also to a well-known spring maker. They none of them happened to have studied the properties of springs. In relation to Case C, I consulted one of our most celebrated electricians. It so chanced that he had never specially investigated the properties of permanent magnets; so in all these cases I labored on alone, having also failed to find the desired information by referring to some of the principal mechanical dictionaries, electrical manuals, and engineer's handbooks. Perhaps the knowledge searched for is known to somebody, and published somewhere, but it certainly is not readily accessible, as it is in the case of the steam-engine. The latter machine has attained a dignity in the mechanical world that has given it a literature of its own, and all the proportions necessary to a good engine can be found given in detail in printed tables. This is, to some extent, true in regard to cotton machinery, and is beginning to be in plumbing work, and a number of other industries.

It will be seen that the main idea attempted in the foregoing remarks is, that the makers and users of machinery in this country should, for their own pecuniary benefit, as well as for the interest they may feel in applied science, *combine* to establish some sort of a *central council* for experiment and research. The *personnel* of this council should include such a number of mathematicians, physicists, engineers, and mechanics, all of the highest ability, as would give it the respect and allegiance of the mechanical public. Its *matériel* would be buildings, apparatus, record books, and the best attainable scientific library. Its *work* would be: First, the publication and distribution of official information regarding any technical subject which the members should think of sufficient importance, and which might be suggested by themselves, or by any

correspondent who needed or desired its investigation; and, second, the fixing of standard sizes and proportions where uniformity of practice is desirable. Its *methods* of work would be literary research, correspondence with practical men, mathematical calculation, and mechanical experiment. The latter, however, could in many cases be dispensed with. To collect, compare, average, and amplify records of other people's experiments and practice would be all-sufficient.

A notable instance of such work was the fixing of the excellent "United States Standard" for bolt threads, nuts, and heads, a few years ago. It was the combined work of *individuals* (the Messrs. Sellers) for their own practice, a *society* (the Franklin Institute) for the promotion of science, and the United States Government, which latter made it semi-authoritative by deciding to adopt it in the Navy merely.

The important questions arise for consideration, *when*, to *what extent*, and *by whom*, shall this work be done? To the first, a natural answer is—*now*. The second depends somewhat upon the third, and upon the money and enthusiasm at command. The third answer is respectfully referred to the American Society of Mechanical Engineers, with the hope that, if the subject should seem of sufficient importance, it will be properly discussed. It may be that your learned body, representing the best scientific and mechanical talent of our land, will now or at some future time, see fit to make a beginning in this desirable work. Should such be the case, the possible methods of action are various. A practicable way might be to secure co-operation, and to bring about a systematic division of labor among the societies and schools that are already at work, thus increasing their efficiency many fold. Independent action might be the better method, and, however small the beginning, a nucleus would be formed, around which would, in time, accumulate the intellectual and pecuniary offerings of a grateful and appreciative engineering public.

Should not the engineers of America, to maintain their credit at home as well as the great reputation they have gained abroad, see to bringing about a time when a peregrinating journeyman will not have to master a new system of hieroglyphics upon the drawings at every shop he works in,—when every shop-owner will not have to select to suit his fancy from a dozen assorted brands in buying a wire gauge; and figure out twenty different sized pulleys to coax on to his line shafting to drive twenty "eight-

een-inch" lathes; and puzzle his brains establishing for himself standard sizes and angles for nut bevels, and machine screws, and key-seats, and loose collars, and drawing-boards; and find in his mechanical dictionary half a dozen speeds, varying some five hundred per cent., as each and all correct for turning cast iron,—when he will not need to build a metal-testing room of his own,—a time, in short, when *one* well-done calculation or experiment shall replace a *thousand* half done, and system shall replace chaos?

DISCUSSION.

THE PRESIDENT: I think that matter is a matter worthy of some debate, and a matter of pretty general interest to us all. I think that after the gentlemen have seen what has been done during the last few months at the Pratt & Whitney Company's works, and have seen what ought to be done at various other establishments that we have visited to-day and at other times, they will come to the conclusion that this work can be systematized by the concerted action of men of adequate knowledge, skill, and experience in such a way that the world would be a very great gainer, and that instead of one or two firms expending twenty, thirty, or forty thousand dollars in experiments in getting results that are only of value to them, and of limited value even to them, we should by a proper systematization of methods get for that same expenditure many times the value, and get it in a satisfactory and authoritative form, and in a form that would be accessible and available to all. This proposal, of course, as you all know, is not a novel one. The matter has been proposed before, has been thought of seriously before, I presume, by every man who has had much to do with mechanical work; and it has even taken promising shape on several occasions; but there have always been difficulties in the way and the result to-day has not been at all satisfactory. Some of the first attempts that have been made to secure practical knowledge by careful and skilfully directed experimentation have been made under the supervision of the government. A committee of the Franklin Institute conducted a series of experiments on the strength of iron many years ago, in connection with the investigation of the cause of steam-boiler explosions, that had great value. The results were published in a public document, which is still obtainable, although rare. The results were of great practical value, and remain valuable to-day. Another investigation, made

just a little later under the auspices of the government, was that of Professor Johnston on the value of American coal, and that document, containing Johnston's report, on American coal, remains to-day one of the most valuable books an engineer can have in his library.

MR. WOODBURY: I have tried to obtain that book—is it obtainable? I have asked both booksellers and correspondents and have been unable to get it.

THE PRESIDENT: An attempt was made a few years ago only, to institute a series of experiments on the causes of steam-boiler explosions that should be complete, exhaustive, and valuable. Congress very liberally appropriated $100,000 and the President was authorized to appoint a board—a commission—which should conduct the investigations. The President was not well acquainted with the men in the country who are capable of conducting such an investigation. There was no Society of Mechanical Engineers to whose officers he could go and of whom he could ask the names of the leading men in the country in the profession, and from whom he might obtain information that should lead to the formation of a proper commission. He did the best thing that he could do under the circumstances, no doubt. In the Treasury Department there exists a bureau, presided over by the Supervising Inspector General of Steamboats. The President made him the chairman of this commission, and appointed a body of men whom he supposed were competent to conduct the investigation and the matter was left in their hands. They at once proceeded to spend money very freely—laid quite large plans; but for causes that need not be mentioned here the expenditure of money was not as wisely made as it might have been. A large proportion of the appropriation was lost from that cause, and after various mishaps—some due to fault and some to misfortune—the board died an unnatural death, leaving their work incomplete. Some work was done—some interesting work was done—but the board has never made a report. The organization changed in form and changed in members. Some distinguished men were on the board at intervals, but the result had been nil. No report exists. Notes were taken by the members of the board, and I presume those notes are in existence. I was on the board for a time until my health failed; and for that and other reasons that were obvious to me I left, and during the period in which I was connected with it, I

know the experiments were conducted carefully so far as they went. The notes that were taken I am confident are in existence and I presume a concerted movement would bring out those notes from those members of the board who are still living, and reports by members to the Treasury Department. If such reports were made, they will be published as a matter of course, and the public document containing reports so given would then become accessible to all. But to-day we simply can look back upon the expenditure of $100,000 nominally to ascertain the causes of steam-boiler explosions, with but little result. If that thing were attempted again, if the same opportunities were offered to-day, I think it is extremely likely that results might be obtained that would be very valuable and more than commensurate with the expenditure. I presume that under similar circumstances the President of the United States and his advisers would look to a body like this Society for advice as to who should be appointed on such a commission and as to what direction to take, perhaps, as to methods of investigation. But the non-success of the board, I have no doubt, has hindered investigation in that direction to such an extent that none of us here present will ever see the matter reopened. I presume the investigation into the causes of steam-boiler explosions, even were it to be considered as necessary as it was thought to be then, will not be again undertaken in a generation.

Fortunately other work, especially of the Hartford Steam Boiler Inspection and Insurance Company, and the works of similar companies in Great Britain, has enabled us to acquire knowledge that could not have been acquired even by such a commission. In the course of their business operations they have been compelled to study up the subject. They have had opportunities of observation and investigation that no government commission ever could have obtained; and very fortunately, therefore, as I say, those commercial bodies are acquiring information of great value, and the causes of steam-boiler explosions are gradually becoming known; and I suppose all engineers who have watched the progress of their investigations and studied the results of their work, have come to the conclusion that there are three principal causes of steam-boiler explosions; at least I myself have no hesitation in attributing the great majority of them to three principal causes: the first is ignorance, the second is carelessness, and the third is utter recklessness. Those are *the* three causes of

steam-boiler explosions. The number of steam-boiler explosions of which the causes remain unascertained is a very small percentage of the total number, perhaps four or five per cent. I do not know what the figure is precisely, but it is very small, and those are principally cases where lack of knowledge comes simply from lack of opportunities of observation. So that it may be stated as a positive fact, I can say, that we know to-day, that steam-boiler explosions can be attributed simply to easily preventible causes, and the work of such a commission is not to-day as much needed as it formerly was. It remains possible that there are causes of steam-boiler explosions which are very rarely operative and which still remain undetermined, perhaps unsuspected; but they are so rare, that they have no direct value—no direct importance, I should say. Another attempt was made a little later to make a series of investigations under the auspices of the government, which resulted more favorably, but still not as favorably as we might wish. A committee of the American Society of Civil Engineers first took action several years ago—I think it must be ten years ago now—toward the creation of a government commission to investigate the strength of American materials. They have a standing committee—you will find their names printed on every issue of the Transactions of the Society, on the first inside page of the cover—a standing committee on the tests of American iron and steel. The object of that committee was to secure the appointment of a commission and the inauguration of an investigation, such as Mr. Smith has suggested here to-night.

After some years of somewhat ineffective work, their efforts were finally successful, and Congress directed the President to appoint a board to make tests of iron and steel, and other metal, and to report results. That board was to consist of an engineer officer of the army, an engineer officer of the navy, an ordnance officer of the army, and three civilians. This board, so constituted of persons who were expected to be experts in the direction that the investigations were to take, was appointed by the President accordingly, and Congress made an appropriation of $75,000 to do this work, with a proviso, as the bill first was passed through the house, that $15,000 should be used for the expenses of the board, and that $60,000 should be appropriated to the construction of a machine. In the meantime the Committee of the Society of Civil Engineers, who had been acting energetically with the appropriation committee to secure the appointment of the board, found that some influ-

ence was at work that they had not known anything of, and that influence had secured this peculiar wording of this resolution which was to be a joint resolution of both houses; but, by their action, and, possibly, by the action of friends unknown to them, the wording was finally changed, and an appropriation was made of $75,000, which was to be used at the option of the board in their work. Part of the wording still remained as before; that is, they were allowed the use of $15,000 for the commission. The interpretation naturally given to that was that it was to be used in paying expenses of the commission, travelling expenses and incidental expenses. The board met immediately after its appointment at the Watertown Arsenal, and received there, at a subsequent day, plans for the construction of testing machines, with specifications and prices that were named. They selected a plan which seemed to them the best, directed the construction of such a machine, and appropriated the required amount of money for it. The contract called for an expenditure of $31,500 on the machine. They were informed that the chief of ordnance (as this machine was to be placed at the Watertown Arsenal, and would fall into the hands of the Ordnance Bureau when the board had completed its work), would put in the foundations of the machine, and thus save the board a considerable amount of expense. But that was not stated officially; and ultimately those foundations were put in at the expense of the board, so that the major part of the appropriation of the first year was expended in the construction of a testing machine.

But, while waiting for the construction of this testing machine, which was intended for the testing of very large masses of iron and steel, the board went into subsidiary investigations, as they considered them, intending to make the more important investigations,—the investigations into the strength of structures and large masses of iron and steel,—after that machine was completed; and, so long as that appropriation remained in hand, they continued their work there, and they expended the full amount of the appropriation upon the machines, or upon these investigations. The amount used in the personal expenses of the board amounted to very little. The members did their work as best they could, and at an expense that was insignificant, outside of actual cost of making tests. The result of the work of the board, so far as it was carried out, was published in a public document in 1878. That document can be found by members during the coming summer at Washington, and I believe it can be procured

by application to your representatives. But the appropriation, of course, was soon exhausted, and Congress gave another small appropriation the succeeding year.

But after the machine was completed, and after these investigations were well under way, and the board was just in good condition, in every respect, to go on and do work that should be creditable and valuable, Congress declined to make any appropriation, even for the use of the machine that they had built, and the board died in consequence of the expiration of its appropriation. The limit of life for the board was fixed by the limit of its appropriation. When the appropriation expired the board ceased to exist. So the board went out of existence just when it was getting ready to do its work, and to do good work; what it could have done gentlemen can judge very well by reading the report which will be published this summer. In that report you will find what was done with about fifteen or twenty thousand dollars. The financial statement is in the report, and you can judge for yourselves how much that work is worth, and how well the expenditure of the board has been repaid by the acquisition of knowledge. But Congress seemed to have no appreciation of the importance of that work and declined to do anything for the board. An immense amount of influence was brought to bear upon the appropriation committees, but without the slightest effect. Memorials were sent in by the American Society of Civil Engineers; by the Society of Mining Engineers; by the iron and steel associations; by the faculties of all the prominent technical schools; by the faculties of some of the best known colleges; and recommendations were made by a large number of well-known business men, and influence brought to bear upon the appropriation committees by members of Congress from all parts of the Union. Some gentlemen worked very earnestly, and yet an amount of influence that would naturally and ordinarily secure the appropriation of almost any amount of money, and carry through Congress any reasonable,—any at all reasonable,—proposal, failed to secure another dollar of appropriation for the board.

The machine, when completed, came into the hands of the Ordnance Bureau of the army, and is now in use by them doing good work. An appropriation was secured by the Ordnance Bureau, at the last session of Congress for the continuance of work with that machine, and there seemed to have been no difficulty in securing that appropriation; but the influence of all the

business men in the country, the influence of all the scientific associations in the country, the influence of all the faculties of the technical colleges in the country combined, could not succeed in getting the appropriation. So that gentlemen can see what is to be done if they expect to accomplish anything further in that direction. So long as the interests of the community seemed to lie in the direction of the production of a testing machine simply, there was no difficulty. When it seemed likely that the board would be able to use that machine effectively, there was difficulty; and I presume the conditions remain to-day as they were then. Those are the ways in which attempts have been made; and I have indicated about how much success has been met with in the way of securing effective scientific work that would be valuable to the business men of the country, under the general administration of the government. If the attempt is made to secure such work outside of the executive departments of the government, you will find the difficulty still greater. Members of Congress do not like to put money into the hands of irresponsible parties. It is much easier to get money appropriated for use by a department of the government than for any work to be done outside; and the only chance in this case was to secure the co-operation of the government officials with civil appointees.

I am taking a great deal of time, but I would like to say a few words about some other work that has been attempted. If the gentlemen will bear with me I will go on for a few minutes longer.

SEVERAL MEMBERS: Go on.

THE PRESIDENT: A few years ago two or three prominent gentlemen connected with our railroads came to me and asked if some such commission could not be formed, if some such method of doing work could not be inaugurated; or if we, at the Stevens Institute of Technology, at Hoboken, could not ourselves start in a small way some such investigations as have been called for in the paper just read. I saw no reason why it should not be done, and told the gentlemen if they would give us the necessary capital and allow us time to do our work well, that we would accomplish anything in that direction, and I myself had no objection at all to making the attempt. I saw the trustees and they naturally were very glad indeed to lend a hand in the matter, and the matter seemed to have been agitated in various directions. Members of the Society of Civil Engineers spoke of it, and took official action in the matter in their meetings; and a good many individuals at

about that time seemed to have taken very much interest in the subject. That focused the movement at the Institute, and inaugurated what we called the Mechanical Laboratory of the Stevens Institute of Technology. I had no funds, I had no assistants, I had nothing but the countenance and the interest of these gentlemen. But I proclaimed that we would establish a Mechanical Laboratory at the Stevens Institute of Technology, and went ahead. Fortunately, at this time, the government board had just been instituted, the commission of which I have just spoken; and as chairman of some of the committees of that board, I was directed to make certain investigations. I simply took the apparatus of the Stevens Institute of Technology, and for a time appropriated it to the use of the board; found some bright young men who had gone through the course, had graduated creditably, and shown themselves skilled in manipulation, and put them at work; and with, of course, a good deal of supervision on my part, but with active, earnest work on theirs, we succeeded in doing a large part of the work that actually was done by the government commission. A good deal of work was done outside. Mr. Holley did a good deal; General Smith did some. A large amount of very valuable work was done by a committee consisting of Commander Beardsley and some other gentlemen, in the investigation of the properties of iron; our Mechanical Laboratory took charge of a certain amount of that work, and that was a starting-point.

I borrowed money where I could, and I begged money where I could; and where I could not do either, I took it out of my own pocket. But in various ways I accumulated apparatus and testing machinery, and set going the Mechanical Laboratory of the Stevens Institute of Technology. Well, the amount of work done there amounts to-day to about $40,000 worth of experimental work. That is direct scientific investigation, and directly in the line that is indicated as desirable in the paper that has been read. But my duties and the work that I had accepted from outside professional practice, proved to be too much of a load for me, and I broke down; and during my absence from the Institute the work done by the laboratory naturally became less and less. My colleagues took a very earnest interest in what was going on, and much work was still done; but the amount of work became gradually less and less, until on my return I found very little was being done, almost nothing, in the direction of investigation; and since I have been back I have not had the strength or time to push the experiments

as I did at first. We are now doing a small amount of commercial work, making examinations of the strength of materials for the Dock Department of New York, the Erie Railway, and private parties in all parts of the country. But it is purely commercial work. It does not lead up to what Mr. Smith asks for: the scientific determination of laws and facts in such form as to be accessible to the public. And I am not very certain that as matters go now I can re-establish that adjunct to my department on the basis that I hoped to put it upon. If I get strength, and if friends assist us in an interested, active, earnest way, I have no doubt we could find funds enough to endow it. But it requires work; and one man, I find, cannot do more than about three men's work. Consequently the success of such a scheme depends, you see, not only on the interest of the members of the profession, but on the activity that that interest inspires. The whole thing is perfectly feasible. The plan of making such investigations in the manner which is always expected in scientific work can be carried out. It simply requires brain, physical strength, and capital; and if the Society can find a way of bringing those things together it will accomplish results that will be simply wonderful.

Mr. Holley: I would like to add one word, Mr. President, to what you have said. I could say a good deal upon the subject, but the time is passing rapidly. It must be obvious to the Society that the Ordnance Department of the United States Army does not wish to co-operate with that perfect harmony with civilians that might, under some other circumstances, have been expected, not to put it too strongly. Seeing that the Ordnance Department may not wish to go into that co-operation with civilians in conducting these experiments, but that it desires to control that matter itself, if that is the only way in which it can be made to help us in this work, then, certainly, it becomes the duty of the mechanical engineers to try to stimulate the Ordnance Department to make experiments that will be useful to us and the industrial arts generally, and not useful merely to the Ordnance Department. I just throw out that mere hint.

The President: And I would add to my remarks on the work of the United States commission appointed to test iron and steel, that the discovery by the president of the board of the inventor of that testing machine, Mr. Albert Emery, is enough of itself to justify the creation of that board, and the expenditure of all its money. I think the discovering of Mr. Emery was one of

the greatest discoveries of the age; and the construction of the testing machine has been one of the greatest pieces of engineering work that ever has been done. That machine has done and it is doing its work; and if nothing more has been done by the board, as I said a moment ago, that is a great deal, fully enough to justify the creation of that board, and the expenditure of all the money that has been and will be expended upon that machine. The machine is open to the use of the public, and it is being used to-day very largely, and is in almost constant use by our business men. And I would say, too, that although I do not feel at all satisfied with the results of my experiments in the establishment of a mechanical laboratory, I think that our success, so far as we have obtained results, has been quite sufficient to repay all the expenditure of time, health, energy, strength, and money that has been made on it.

Mr. Stirling: I would like to call the attention of the gentlemen to another way in which we can get the information, to some extent, that has been asked for. Having the good fortune to be a lieutenant of Mr. Eckley B. Coxe, of Pennsylvania, I have the privilege of being under the same roof with one of the finest technical libraries in the country; and in that library we have a book which is published by the German government,—I do not know what bureau of that government,—and that book gives a statement of every article that is published on every subject in every country. And as an illustration of what good this is to us, the other day I had occasion to look up the subject of the transmission of power by friction gearing. I asked the librarian to give me all the literature there was on the subject, and I got a list of thirty or forty articles, published in different languages, on the subject of the transmission of power by friction gearing. I think that in that way gentlemen can be posted upon a great deal of this experimenting that has been done by individuals, on almost every subject.

Mr. Smith: I would like to say a word more, if it will not take too much time; as this is a subject on which I feel very deeply. I feel that I am too young a member of the Society to make a motion on the subject, and shall not do it to-night. But I think that a committee should be appointed to consider the question, and report at a future meeting, whether anything can be done by this Society, or whether the matter should be left entirely alone. If, however, anybody here wants to make a motion

I shall be very glad. What is wanted is not only the ability to get at the technical books and articles that have been published on the subject, but a brief *résumé* of them. An average manufacturer cannot afford to search through a half dozen learned books, even if he can get them, and collect all the information that is given there and condense it. He wants to be able to correspond with a standing committee of this association, or some other that is known as a standard throughout the country, and get at the best figures, which need not be exactly accurate, something just to guide him so that he will not go too far astray on any particular thing he is working on. It is useless to hope, as our President says, for much money to be spent by the government; still we can all do what we can in that direction, by bringing it before Congress and friends who have influence there. Whatever is gained will be gained by independent work; and although it may not be much now, on account of the want of means in this Society, yet the Society will grow and we will get more means, and this expense might, perhaps, be paid by the members. It would not be a very great expense to keep up an organization with which people could correspond and which would give the results of what has been done. After awhile it would grow to be of such importance, that it would be a standard for working from by all progressive men. And, something I did not mention in the paper, that is wanted greatly among our mechanics, is a standard of nomenclature. Great confusion now results from having half a dozen names in different machine shops for the same thing. That, and standard sizes of gauges, and the collection of needed information, and the answering of questions regarding what has already been done, would not be such an immense work, and could be done at comparatively small cost. Although I do not think the Society is large enough to undertake it now, yet we can all use our utmost endeavors to make the Society grow, get membership of the right kind, more money in the treasury, and after awhile we shall see the importance of this subject so clearly as to be willing to spend a little of our money. I shall, certainly, at another meeting bring about some kind of a motion for a preliminary committee to investigate the subject more at length, if it is not done now by somebody.

The President: The accomplishment of anything in that direction will require a great deal of careful thought, preliminary work, and cautious procedure. It involves a good deal more than

gentlemen generally are disposed to anticipate. It means the devotion of some man or men exclusively to a certain object; and if a manufacturer cannot afford to give the time to the looking up of half a dozen references, it is doubtful if he can find any other man to give his time to looking up a hundred references for a hundred different persons. To get good work done requires the expenditure of a good deal of money; but it is a matter that has been deemed of sufficient importance to be called to the attention of other leading societies in the country; all the technical societies and faculties of technical schools have considered it as of great importance; and I have no doubt that with a special and concerted action, the time will come when the thing will be established. Referring to Mr. Stirling's remarks, the work he refers to is *Carl's Repertorium*, and it was published for quite a long series of years in Germany, by the editor Carl; and he was succeeded by Schubarth, so that the late issues are called *Schubarth's Repertorium*. Gentlemen interested in investigations, who wish to look up references, by obtaining a set of that work, will put themselves on the track of about all that has been done in the direction of scientific and technical research. And then in reference to what has been done in this country, turn to the files of the *Journal of the Franklin Institute*. I do not know how many volumes of that have been published, perhaps sixty or eighty volumes, but it runs back a great many years, and contains an account of almost all the important work that has been done in this country. The *Philosophical Magazine* gives an account of the greater part of the valuable scientific work done in Great Britain. The *Annales de Chemie et de Physique* tells you what has been done in France; and you will find if you go to the Astor Library, in New York, that the librarians can always put you exactly on the track of what you need if it is published at all. *London Engineering* is to the engineer a perfect mine, and a mine you will never tire of working.

XXV.

THE CONTINUOUS ROD MILL OF THE TRENTON IRON COMPANY.

BY WILLIAM HEWITT, TRENTON, N. J.

THIS mill was designed by Mr. Charles Hewitt (my father), since deceased, and operates on what is known as the Belgian system—that is, the billet is first roughed in a set of breaking-down rolls, and from them conducted through a series of passes in a train of smaller rolls, driven at a higher speed, and connected in the ordinary way by wabblers and coupling boxes, the peculiar feature of the system being that the rod, as it issues from each pass in the latter rolls, is turned by hand and entered in the next succeeding pass, so that it is operated upon in several passes at the same moment, as in a continuous mill. Very long rods can be rolled in this way, weighing from eighty to over one hundred pounds.

But the mill owes its peculiar interest not so much to the system of handling the material, as it does to the manner in which it is driven and the simplicity of its mechanism. With the exception of the roll-pinions it contains no gearing. The breaking-down rolls are coupled direct to a high-speed Corliss engine, and the train is driven from the fly-wheel through a double leather belt twenty-six inches in width, made by J. B. Hoyt & Co.

The mill embraces also two heating furnaces, with a peculiar arrangement of boilers attached to them, on which the company own a patent, a fan for supplying the blast driven by a small independent engine, a pump and a feed-water heater. Connected with the mill also are two 60-horse power Babcock & Wilcox tubular boilers; but these, with the furnace boilers, are more than sufficient to supply the necessary amount of steam, and the surplus goes to other parts of the works, the boilers throughout communicating, so that these should hardly be included as a necessary part of the plant. At the same time, the furnace boilers alone are scarcely adequate, as the furnaces are damped at each fresh charge of billets, which affects considerably their steaming capacity. If the furnaces were fired for steam only, the boilers over them would probably do the work, and, indeed, the mill has been run with them on several occasions, independently of other boilers, but the steam pressure dropped gradually from 75 to about 30 pounds. At one time, in attempting to roll steel at the latter pressure, the engine

was stalled. The mill, however, will roll iron, if well heated, with a steam pressure of 35 pounds.

The furnaces are of the ordinary kind, fired with bituminous coal, and consume about half a ton (2000 pounds) to the ton (2240 pounds) of rods. The boilers consist simply of plain cylinders, *A A* (see Figs. 13, 14 and 15), two in number, over each furnace, 29 feet in length and 3 feet in diameter, with short drop cylinders *B B* of the same diameter, suspended from the ends adjoining the stack, 6 feet in length and filled each with fifty-six 3-inch tubes. The flame from the furnace, conducted by the uptake *U*, strikes the ends of the plain cylinders *A A* furthest from the stack, passes along under them about three-fourths of their length, then drops into a chamber *C*, which acts as a kind of combustion chamber, passes through the tubes of the drops *B B*, and thence up the stack. These boilers, judging from the quantity of water they take, generate about thirty per cent. more steam than plain cylinders alone would generate similarly placed. The heat from the furnaces is so well utilized that a man can put his hand in through the doors *d d*, just behind the drops at the foot of the stack, without much inconvenience, while the furnace is in operation. No flame ever issues from the top of the stack, and the damper *E* is placed at the foot just above the boilers. Each pair of boilers joins in one steam drum *D*, and doors *d d*, directly in front and behind the drops, are provided, so that the tubes can be readily cleansed, which is done every Saturday.

A Niagara pump (No. 4), in connection with the feed-water heater, supplies the water both for the furnace and Babcock boilers. The heater consists simply of a large rectangular cast-iron box, containing about 430 feet of 1¼-inch wrought-iron pipes, the water passing through the pipes and the exhaust steam from the engine around them, feeding the water to the boilers at a temperature averaging from 185° F. to 200° F. In the previous heaters used at the works the steam passed through, and the water around the pipes, but for some reason which I am not quite able to explain, this disposition of the materials caused the pipes to corrode quite rapidly, becoming completely honeycombed in the course of a few months, so that they had to be frequently renewed. With the present heater, however, the first renewal has yet to be made, and the pipes are still in fair condition.

The most interesting feature of the mill, perhaps, is the engine, which was built expressly for the purpose by the Corliss Steam-En-

gine Company, of Providence, R. I. The diameter of the cylinder is 20 inches; length of stroke 42 inches, and number of revolutions 160, giving a piston speed of over 1100 feet per minute. It operates on the general principle familiar to all, but the mechanism is modified somewhat to accord with its speed and to insure a more prompt action of the valves, all the parts fitting loosely and yet being rigidly secured. Small sensitive springs *s s* (see Fig. 16) operate the latches *l l*, lifting the valves *v v*, which are thrown out at the proper moment by trips *t t*, actuated from the governor, and the dash-pots are so arranged as to allow of a more rapid escape of the air from underneath the plungers at the moment of cutting off, being pierced with several small holes, that are plugged up one at a time as the plungers wear, so that the latter may not drop too quickly.

The exhaust port is eight inches in diameter, expanding into a 12-inch pipe. The latter originally was only 8 inches in diameter, but the back pressure choked the engine too much, and it was subsequently changed.

The connecting-rod brasses are provided with a peculiar arrangement of set-screws that prevents them from clamping the pins too tightly, and, at the same time, allows them to be rigidly keyed to the rod. The connection with the cross-head is lubricated with oil by a hole in the centre of the wrist-pin, communicating with a number of small radial holes opposite the connecting-rod bearing. The crank-pin, however, requires the best white tallow. Ordinary lubricating oil is too thin, and would be thrown out in all directions. The cup holding the tallow contains a small loose copper rod, that plays up and down against the pin and the cap of the cup, for the purpose of working the tallow down, and also serving as a conductor of heat to melt it more freely as the parts become warm. This copper rod is quite essential to the ease of the crank-pin, and performs its functions so well that the pin never becomes overheated, but keeps just moderately warm all the time. I regret that the haste with which this paper was prepared has not allowed me to present a detailed illustration of these devices.

The crank is of the disk-wheel pattern, carefully balanced, and the shaft is 8 inches in diameter. The fly-wheel is in one piece, 10 feet in diameter, and weighs something over 16,000 pounds. It has wrought-iron bands shrunk upon each side of the hub. The small pulley driving the train is 54 inches in diameter, and weighs a little over 4000 pounds. The faces of the wheels are 28 inches width,

and the distance between their centres 16 feet. The centre of the small pulley is a little below that of the fly-wheel, on account of the train being lower than the breaking-down rolls, in order that its driving shaft *S* may pass under the floor behind the latter rolls, and thus give a clear space for the iron to play upon.

Since the mill was started several improvements have been added to the original design of the engine, but not in any way affecting the principle of the valve motion. The eccentric rod, as first arranged, frequently shook loose from the valve gear, notwithstanding the latch provided for securing it; and several devices were tried without successfully meeting the difficulty, until the present latch (see Fig. 20) was applied, which was designed by James Withington, the foreman of the Trenton Iron Company's works.

The original eccentric threw the oil out, so that it was impossible to keep it cool. This difficulty was overcome by altering the strap (see Figs. 18 and 19), so as to confine the eccentric and make it impossible for the oil to escape.

The valve supports *S S* (see Fig. 17) broke, and the support *o* for the wrist-plate gradually worked loose, the former being replaced with heavier castings, and the latter stayed at the end just outside the plate by a small standard.

The first tightener consisted simply of a lever, with the fulcrum at one end resting between two upright cast-iron columns and the pulley at the other supported by the belt, the lever cushioning upward against two nests of car springs, inclosed in cups, secured to the main standard by rods. The fluctuations of the belt were such, however, as to cause the pulley to thump seriously against it, so that it had to be continually taken up. In the present tightener (see Figs. 21 and 22) the lever is provided with arms *A*, extending from the opposite side of the fulcrum *F*, and also cushioning against nests of car springs *N N*, like those on the pulley arms *P P*, and similarly attached to the main standards *M M*. These relieve the belt from the sudden shocks to which it was before susceptible, and at the same time allow to some extent for its fluctuations. The arrangement works so nicely, indeed, that the motion of the arms is almost imperceptible.

Since these alterations were made the engine has given no trouble whatever, and the repairs on account of wear and tear have been comparatively trifling. The valves now need reboring for the first time, but the cylinder is still in good condition, and smooth as a mirror. The belt has required no taking up since the pres-

ent tightener was applied, has stretched but very little, and shows no signs of giving out. It has never slipped but once, and that was owing to the carelessness of the engineer, who dropped some grease on it. The little springs *s s* (see Fig. 16) operating the cut-off latches, give out frequently, but several of these are always kept on hand, and the arrangement is such that they can be replaced at a moment's notice.

The pinions, both on the breaking-down rolls and on the train, are of the V-toothed pattern, made by A. Garrison & Co., of Pittsburg. The original set on the former is still in use, but that on the train was replaced by a new set in December last, the teeth being worn so thin that it was unsafe to use them longer.

The roll-neck bearings are made of phosphor-bronze from George K. Tryon, Son & Co., of Philadelphia, and some of the original castings are still in use. Three months was formerly considered a fair life for roll-neck brasses.

The rod as it leaves the last pass in the train is coiled up by a steam reel, on which the Company own a patent. This reel is so constructed that a boy, seizing the end of the rod as it issues from the train, can enter it in the guides without letting go his hold, and attach it to the pins of the reel while it is in motion, so that it is immediately coiled up and not likely to become entangled on the floor. The reel is instantly stopped by pressing on a treadle that shuts off the exhaust and at the same time contracts the pins slightly toward the centre, so that the coil can be easily and quickly removed when it is wound.

The mill has been in constant operation now day and night for four years, or, at least, has run as steadily as most mills of the kind since it was started. During this time it has rolled about 20,000 tons of No. $4\frac{1}{4}$ round rods from $1\frac{1}{2}$ inch square billets, or an average of over 400 tons per month, the largest product for any one single month being 750 tons. The mill is capable of turning out about 800 tons of rods per month, when driven to its full capacity.

DISCUSSION.

MR. STIRLING: I wish to call the attention of the members to a remarkable statement made in this paper—remarkable to me at least. I have had some experience with Corliss engines driving rolling-mills and never saw one yet running as fast as 160, and always supposed there would be trouble in such a case. I know parties who wanted to run Corliss engines at a high speed and

were advised not to try—that it would not be best. I am very glad to know that this Corliss engine is running at such a high speed; it is something that I did not expect could be done.

Mr. Leavitt: I would like to ask Mr. Hewitt if he took any cards?

Mr. Hewitt: No, sir, I have never taken any card or made any test of the engine. I wished merely to call the attention of those present to it. If any members find it convenient to be in Trenton, I should be pleased to show them the engine in operation, and will furnish the facilities for taking cards.

Professor Thurston: I would like to ask Mr. Hewitt how that engine behaved at that high speed,—whether there was any jar?

Mr. Hewitt: No, sir, very little. At first there was considerable jar, but since we widened the exhaust pipe there is very little.

Professor Thurston: When do you get the most jar,—at low speed or high?

Mr. Hewitt: I do not think that there is much difference.

Professor Thurston: I ask the question, because a few days ago I was calculating the speed that would give the best distribution of pressure for an engine of ordinary proportions such as that is, and the figures I got indicated that the engine could be driven at a very much higher speed than even that, and still approximate to the conditions that are aimed at in the Porter-Allen engine where the pistons are made somewhat heavier. And then it is, perhaps, questionable whether the Porter-Allen engine is not really behind the ordinary form of engine in its proportions,—whether in lightening that engine, and bringing it to ordinary proportions, you are not simply enabling the engine to reach higher speeds with less difficulty. I should suppose the experience of Mr. Richards and Mr. Hewitt might be compared on that point with some advantage.

Mr. Richards: I will say that the reciprocating parts are made light for very high speed. It is only for what we call moderate speeds that the reciprocating parts should be made heavy.

The President: That brings out another point: that engines as ordinarily designed are fitted for even higher speed than the Porter-Allen engine is worked at.

Mr. Wheelock: I would like to ask Mr. Richards whether he has not practically abandoned his reciprocating parts?

MR. RICHARDS: I understand that Mr. Porter has not abandoned making the reciprocating parts heavy.

MR. WHEELOCK: I understood the other day that he had taken out his heavy pistons, and I inquired for information that would be correct. Do I understand the gentleman to say that Mr. Porter has not reduced the weight of his piston at Johnstown?

MR. RICHARDS: I cannot say with reference to that.

MR. WHEELOCK: Well, then, I will make the remark that he has.

MR. RICHARDS: I will simply say that Mr. Porter has not in any degree changed his opinion or practice in this respect. Any change made in the weight of the pistons at Johnstown was that incidental only to a change in the character of the packing.

MR. COUCH: Mr. Porter's experiment at Johnstown has indicated to him not so much the desirability of reducing the weight of the reciprocating parts, as taking as much as possible of that weight off the piston and putting it into the cross-head. That is my understanding.

THE PRESIDENT: I came across, a day or two ago, the results of a test made by some of our young men of Mr. Hewitt's boilers, and the figures came out at 78 per cent.

MR. WHEELOCK: Mr. President, as I understand Mr. Hewitt, of the Trenton Works, he has a belt and also a fast-running engine. I would ask what real gain he makes out by increasing the speed to the high rate to which he has just informed us he has increased it? Why would not it be just as practicable to reduce the revolutions and get the piston speed by a long stroke? With a larger pulley and a belt, as you have,—a belt and its inconveniences,—would not it be well to make the pulley larger?

MR. HEWITT: I did not catch Mr. Wheelock's question.

MR. WHEELOCK: I would say, Mr. President, as far as my experience goes, these high rates for rotating speed have been a step backward rather than forward when they have been fairly tried. That is a pretty bold assertion I know at this time, but I make it all the same and am ready to stand by it.

MR. BALDWIN: In this case there are two mills at Trenton. The first one is 12-inch, running direct, and they need 150 revolutions to drive that mill. The other mill is run by the belt, and that is the reason for this arrangement.

MR. LEAVITT: There is another very bad feature about these methods of driving mills. I went the other day to see a mill at

Fall River, and when we went to see the engines they were not there; they had gone out. The fly-wheel had broken and wrecked the engine completely, and part of it had gone out of the buildings. Now if we get an engine such as Mr. Porter is proposing to put in at Willimantic, we shall be rid of all that difficulty; and if an engine is built well enough to run at that speed, it will last as long as any engine constructed. That can be put down, I think, as an axiom.

MR. STIRLING: I think it would be well for us all to watch this matter of high speed closely. I had a very good chance to try it. I have an 18″ × 36″ engine running, doing certain work, and at another place I am putting in a 9″ × 16″ Porter-Allen engine to do the same work. The 18″ × 36″ engine runs 75; the Porter-Allen, 9″ × 16″, runs 280. You may be sure I shall watch the matter pretty closely so as to be able to tell you something about it.

MR. EMERY: In this connection I should like to ask Mr. Richards, or any one informed, if it is considered practicable, in the present state of the art, to run up to 600 revolutions—say with engines of any considerable size?

MR. RICHARDS: I do not know what you would term engines of size; it might be done with engines of 75 or 100 horse-power. I cannot give you any positive information.

MR. EMERY: I did not know but you could give us some information on the subject.

MR. RICHARDS: I have no particular information.

MR. LEAVITT: I want to make one qualification, Mr. President, and that is on pumping engines. I saw at Lawrence, the other day, a 28″ × 48″ engine making 108 revolutions, and running very beautifully indeed. The superintendent of the mills told me it was about 500 horse power. It was attached directly to the water-wheel pinion shaft, and the arrangement was such that the water-wheels were between the engines. They tried first to run the engines without fly-wheels, but they could not. The arrangements so far are very satisfactory indeed. The steam is used from this engine for dyeing, bleaching, and heating, under a back pressure of about 13 pounds, and it is a great deal cheaper than water-power at the rate ordinarily charged for water in Lawrence.

MR. HOADLEY: At present I can say what there is to be said about the engines which are running very satisfactorily, to me at least, on probation at present. They are designed to be used as auxiliary to water-power. They are connected directly with four

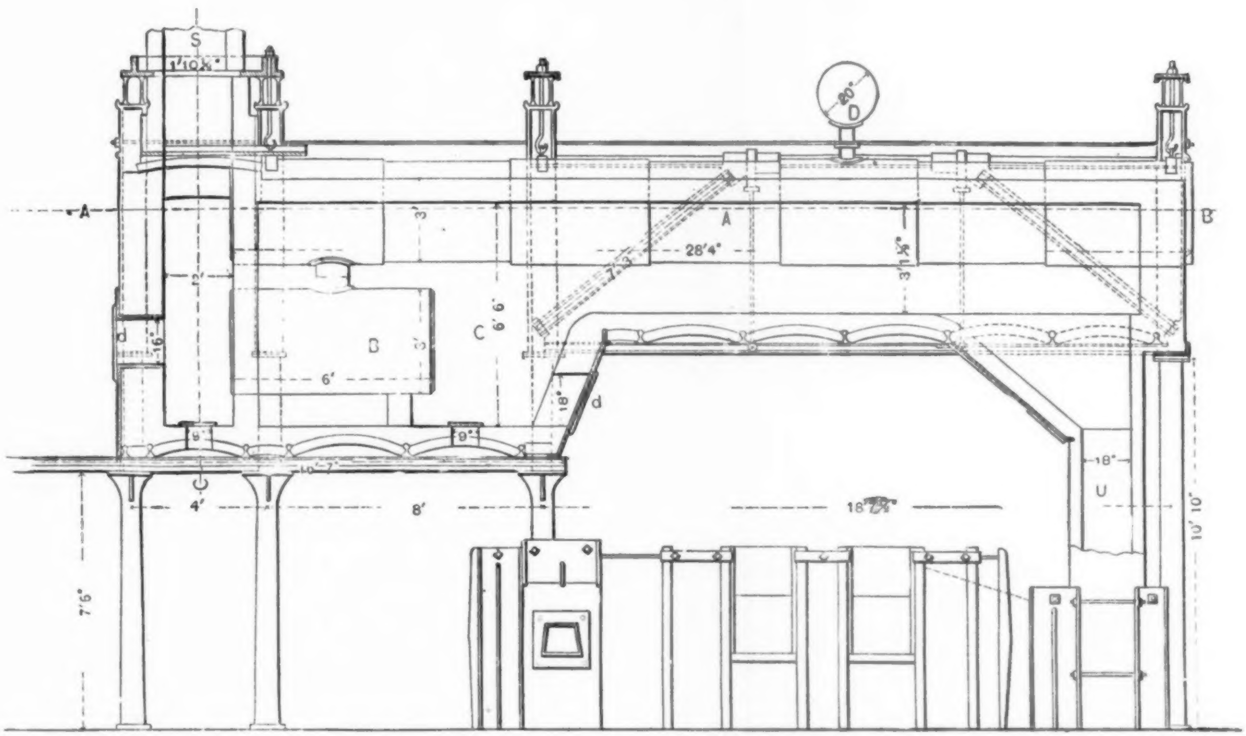


FIG. 13.—ELEVATION OF FURNACE AND BOILER.

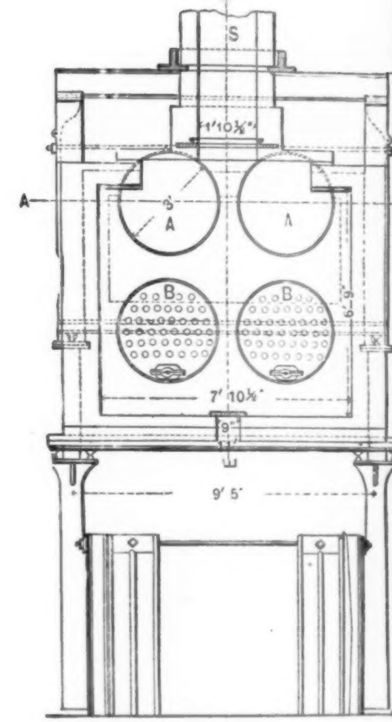


FIG. 14.—END ELEVATION

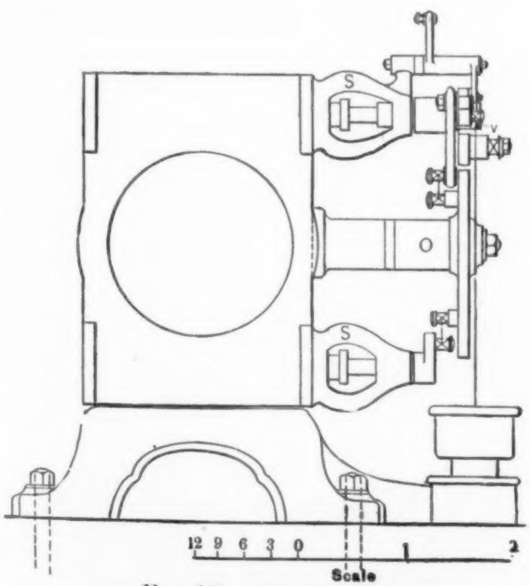


FIG. 17.—END ELEVATION.

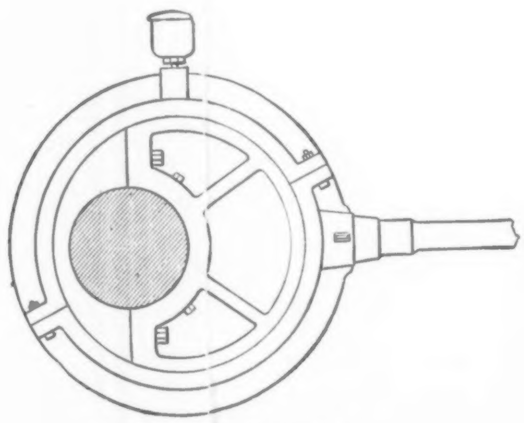


FIG. 18.—ECCENTRIC.

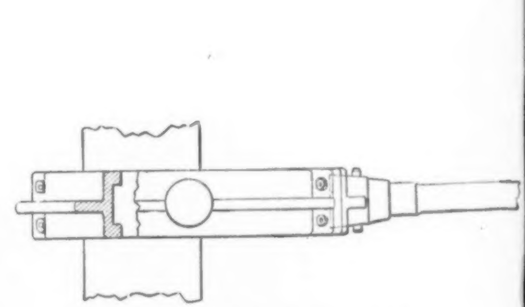
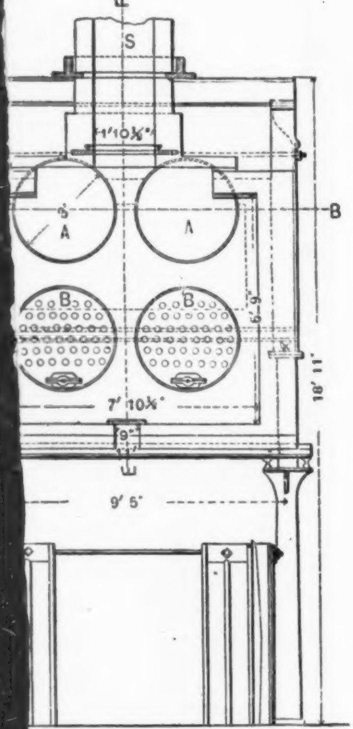
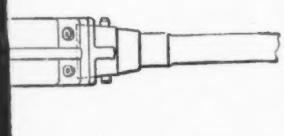


FIG. 19.—ECCENTRIC STRAP.



14.—END ELEVATION



ENTRIC STRAP.

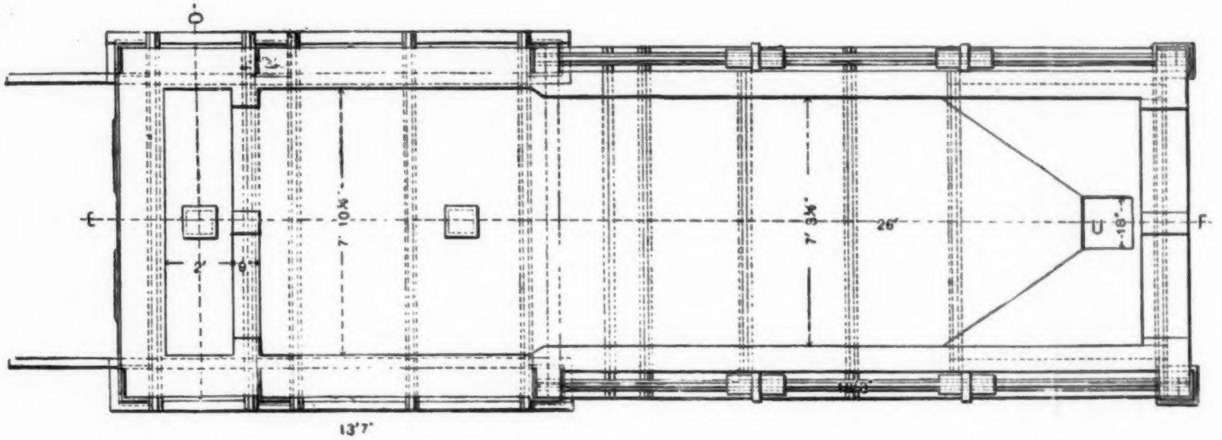


FIG. 15.—PLAN OF FURNACE AND BOILER.

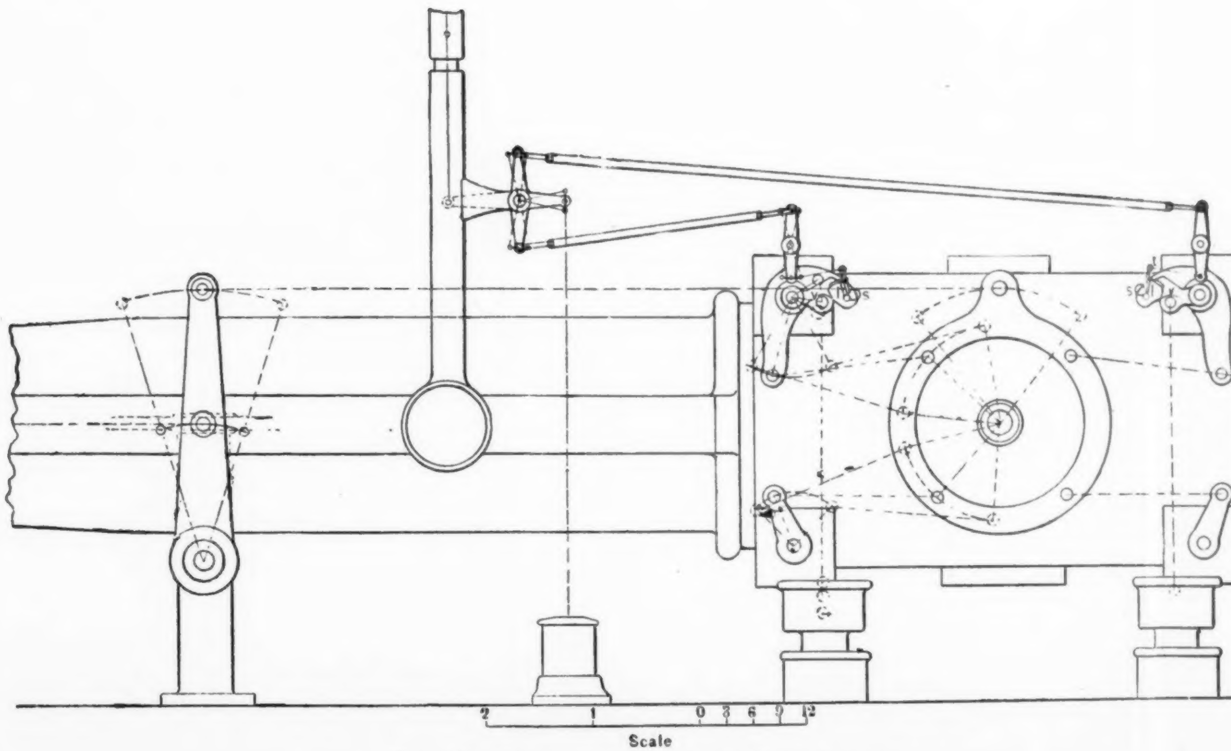
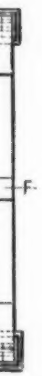


FIG. 16.—ELEVATION OF CORLISS MILL ENGINE.



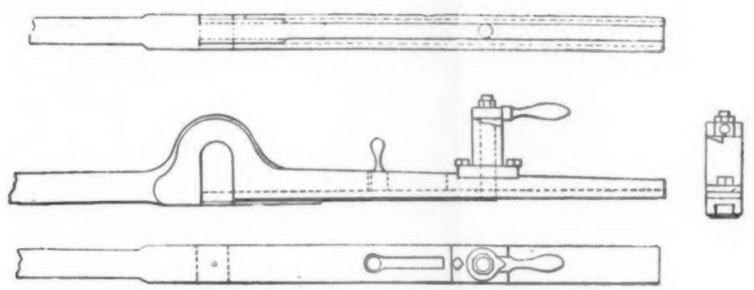


Fig. 20.—Eccentric Rod Latch.

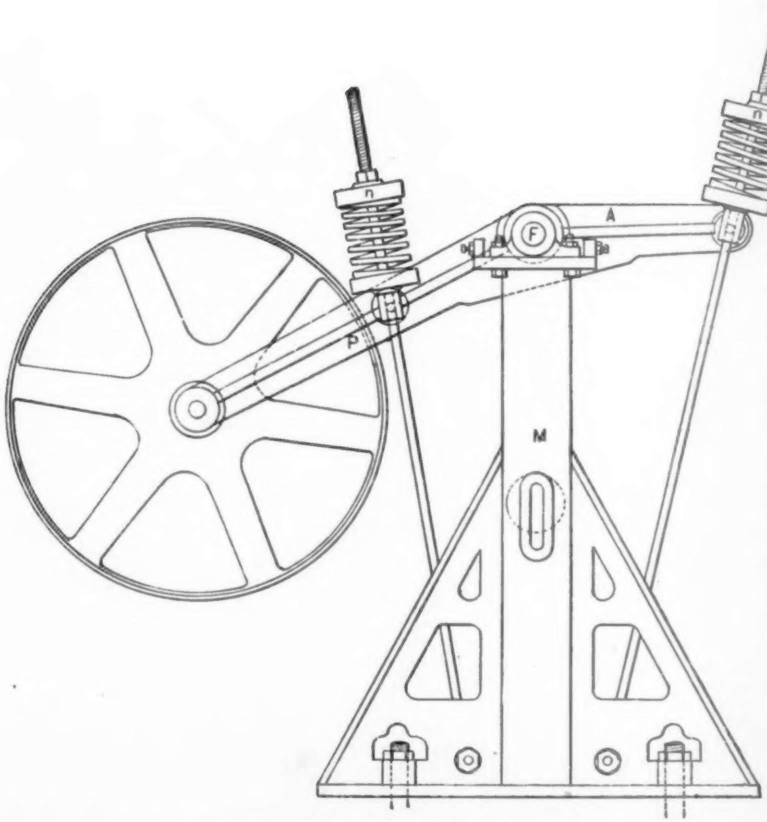


Fig. 21.—Side Elevation of Tightener.

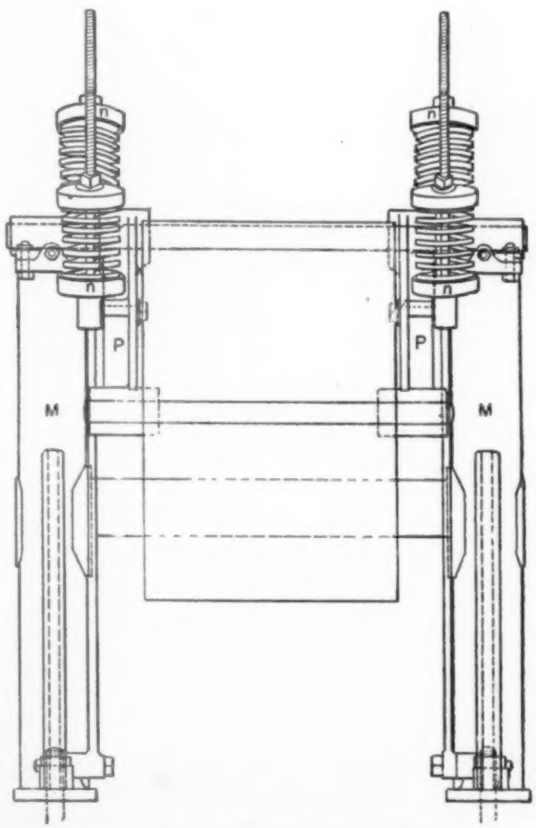


Fig. 22.—End Elevation of Tightener.

water-wheels—four turbine-wheels, 6 feet in diameter, 10-inch gate, of about 280 horse-power net effective each. The engines were built by the Buckeye Engine Company, of 28 inches diameter of cylinder, 4 feet stroke, and connected directly with the horizontal cheek-shaft of the turbine-wheels, and about 85 or 90 feet apart, with the crank set at right angles and in connection with one or more, sometimes all, of the water-wheels. The power of the water-wheels varies very much with the back water. Sometimes when there has been a great deal of back water, the mills have been compelled to stop. At such times the engines, in order to keep up the speed, may develop a thousand horse-power or something over. The economy of steam has not been considered in this case, because at the most, when given under the most favorable circumstances, with 65 to 70 pounds of pressure in the boilers, and with as much as 15 pounds back pressure, the engines developed 500 horse-power when a little more than half filled—sometimes six tenths. But at such times they only use about two-thirds of the steam which the dye-house and the print-works ordinarily require; so that, when passing the steam through the engine at the rate of 46 pounds of steam per horse-power per hour at 13, 14, or 15 pounds pressure, they are simply supplying steam which otherwise would have to be supplied directly from the boilers; and the only loss encountered, so far as I can see, is by the conversion of heat by expansion and so much of radiation as may take place in conveying steam to the engines and from them to where it may be ultimately used. Simply considering them as pipe and also the cylinder as so much pipe; then for all the condensation and re-evaporation in the cylinder, simply so much heat in the exhaust, and therefore as steam rejected by the engine still useful, it is only the heat which is lost through the clothed surfaces of the pipes which is really lost. There were considerable difficulties in making this application. The engine could be speeded so that the couplings when unbolted could keep time with each other very well. It would be necessary therefore to speed it a little above the speed of the wheels, and to try it running through half of the day, disconnect and make the change and try again. There was no opportunity to start the mill with a single engine, to try the engine; but it must be done by running through the half-day and seeing what the result was. The speed, by the way, was said to be 105 revolutions a minute. I have counted it a great many times, three, four, and sometimes five minutes at a time, and I never saw it run less than

106; it is generally about 106 or 107 revolutions. The result I anticipate from it will be about an additional consumption of twelve tons of coal per week, or two tons a day, for the development of about twelve mill-powers of 65 horse-power net to the mill-power, or at the rate of about one ton a week per mill-power—a ton a day for 1000 horse-power; and it seems to me to be a case where economy in the ordinary sense in the use of steam by high expansion, and all the other refinements of construction is to be disregarded, and efficiency of the engine and regularity of speed alone are to be considered. Those conditions I believe will be found to be united in the performance of this engine. I do not know that there remains anything more to be done to its entire success, except to watch and see that it continues to do as well as it has been doing for the past month or two. Of course the success of this arrangement will depend upon the utilization of the steam after it leaves the engine. If the steam is exhausted from the safety-valve at 15 pounds pressure, of course the waste of steam at the rate of 46 pounds per horse-power per hour is very great. There are times in the day when steam has had to be blown into water and condensed and run to waste from inability to use all the warm water; and while that condition of things lasts, there is at certain times a certain amount of waste which cannot be prevented. There are four others which are liable to be clogged at times by the leaves in the water. I have taken out a bushel of leaves from a single wheel. This checks the speed of the mill; this accumulation of leaves in the wheels diminishes their efficiency and regularity. The engine of course has to make up the speed and it calls for a great deal of steam. The regularity of speed and the higher speed on the whole in the case of back water will be due and must be credited to the engine.

Mr. Stirling: The point suggested by Mr. Leavitt is a very interesting one and will merit a little discussion, and that is the question of pumping. We cannot run our pumps at a high speed. One hundred feet a minute is as fast as we can raise the water with economy, and yet we know that that speed is very much too slow for our steam pistons. In thinking this matter over, it has seemed to me that for a moderate-sized pumping engine, the only way to bring the two things so that they will be as we know they should be, is to put some sort of gearing in between the steam engine and the pump. An ordinary tooth gear seemed to me to be the best arrangement to make for a moderate-

sized pump — say a pump pumping a thousand gallons of water per minute.

Mr. Leavitt: I presume Mr. Nagle can give us some information on that point.

Mr. Nagle: Such an engine I designed for the High Service Pumping Works in the city of Providence and worked very successfully; a duty of eighty-six million was obtained.

XXVI.

STANDARD MEASUREMENTS.

BY GEORGE M. BOND, HARTFORD, CONNECTICUT.

The subject of standard measurements is not a new one, though it has received the attention of minds well qualified to master it; still, the lack of a definite system of uniform sizes for general use, especially in machine construction, led to the appointing of a committee by the Master Car Builders' Association to select some one prominent firm engaged in tool making, to undertake to furnish standard United States, or "Franklin Institute" thread screw gauges.

The choice fell to the Pratt & Whitney Company, of Hartford, Conn.; and in order to commence aright, the services of Professor W. A. Rogers, of Harvard College Observatory, Cambridge, were enlisted for the purpose of obtaining an exact transfer from the British Imperial Yard, thus enabling the company to feel assured that the "bottom" had been reached, and to do, once for all, and for the benefit of all, what seemed absolutely necessary for a correct beginning.

The necessities growing out of the difficulties of subdividing the yard, and of applying such subdivisions in practice, led to the construction by them of a comparator, of the form which Professor Rogers found best adapted to comparison of standards. Two of these comparators, or "measuring machines," have been made; one to be placed in position at Harvard College, and the other to remain at the works of the company for use in future comparisons.

It is not the intention in the present paper to give an exhaustive report, or a detailed account of the condition, at this late day, of the question of standards of length, but simply to furnish,

in a brief and general way, such facts and statements regarding the subject as are of importance to those interested in the adoption of a uniform standard of size in the manufacture of tools and machinery requiring interchangeability of parts, and to show in what the standard for the basis of future measurements consists, and the method adopted for determining how closely in practice such standard measurements may be applied.

As is well known, three natural units have been proposed as the basis of standards of length, as follows:

I. The length of a pendulum beating seconds is a vacuum, at the level of the sea, in the latitude of London.

II. One ten-millionth part of the quadrant of the earth's circumference.

III. The length of a wave-length of given refrangibility.

The first of these natural units was found to be unsuitable for the accurate restoration of the original British Yard, rendered useless by the great fire, October 16th, 1834, which destroyed both houses of Parliament, where the standard had been kept.

Sir Francis Baily, Bessel, Kater, and Dr. Young found serious errors affecting the comparisons originally made between the bar marked "Standard, 1760," and the exact length of a pendulum beating seconds under the above conditions.

It may be interesting to here insert the act legalizing the standard:

"SECTION 1. Be it enacted . . . . . that from and after the first day of May, one thousand eight hundred and twenty-five, the straight line or distance between the centres of the two points in the gold studs in the straight brass rod, now in the custody of the clerk of the House of Commons, whereon the words and figures 'Standard Yard, 1760,' are engraved, shall be, and the same is hereby declared to be, the original and genuine standard of that measure of length or lineal extension called a Yard; and that the same straight line or distance between the centres of the said two points in the said gold studs, in the said brass rod, the brass being at the temperature of sixty-two degrees Fahrenheit's thermometer, shall be, and is hereby denominated the Imperial Standard Yard.

* * * * *

"SEC. 3. And whereas it is expedient that the said Standard Yard, if lost, destroyed, defaced, or otherwise injured, should be restored to the same length by reference to some invariable natural standard; and whereas it has been ascertained by the commissioners appointed by His Majesty to inquire into the subject of weights and measures, that the said Yard hereby declared to be the Imperial Standard Yard when compared with a pendulum vibrating seconds of mean time, in the latitude of London, in a vacuum at the level of the sea, is in the proportion of thirty-six inches to thirty-nine inches, and one thousand three hundred and ninety-three ten-thousandths parts of an inch.

"Be it therefore enacted and declared, that if at any time hereafter, the said Imperial Standard Yard shall be lost, or in any manner destroyed, defaced, or otherwise injured, it shall and may be restored by making a new Standard Yard, bearing the same proportion to such pendulum as aforesaid as the said Imperial Standard Yard bears to such pendulum."

In view, therefore, of the errors due to the doubtful reductions of the level of the sea, and the estimated specific gravity of the pendulum employed, and also to other important factors, shown conclusively by Dr. Young, Kater, Bessel, and Baily, to be unreliable, the method adopted and employed in restoring the Imperial Yard, was to use standards which had previously been compared with it.

The bars available for this purpose were:

(*a.*) Shuckburgh's scale (0 — 36 inches).

(*b.*) Shuckburgh's scale, with Kater's authority.

(*c.*) The yard of the Royal Society, constructed by Kater.

(*d.*) The Royal Astronomical Society's brass tubular scale.

(*e.*) Two iron bars, marked A_1 and A_2, belonging to the Ordnance Department, and preserved in the office of the Trigonometrical Survey.*

The restoration of the standard was intrusted to Sir Francis Baily, but his death occurring soon after, the work of restoration was committed to the Rev. R. Sheepshanks. Baily had, however, made numerous experiments regarding the proper material to be used, and that now adopted is known as Baily's metal, the composition of which is: copper, 16; tin, 2.5; zinc, 1.

The mean of all the observations taken, in comparing these available standards, led Sheepshanks to assume that "Brass Bar 2," the name given to the working or provisional standard employed in his investigations, was equal to 36.00025 inches, in terms of the lost Imperial Yard, at 62° Fahrenheit.

The Imperial Standard Yard, known as "Bronze 19," or as now denominated "No. 1," was then constructed according to this equation. It was made of Baily's metal, and of the following dimensions:

Length, 38 inches; width, 1 inch; depth, 1 inch.

Gold plugs are inserted in wells sunk one-half the depth of the bar. The graduations are upon these gold plugs.

* Proceedings American Academy of Arts and Sciences. Paper read by Professor W. A. Rogers, on "The Present State of the Question of Standards of Length." Presented, April 14th, 1880.

"Bronze No. 1" is the national standard yard, and is kept in what is known as the "Strong Room" of the Old Palace Yard, in London.

Besides this bar, four Parliamentary copies were made, one copy being kept in the Royal Mint, one in charge of the Royal Society, one at the New Westminster Palace, and the other at the Royal Observatory at Greenwich. Of the forty copies prepared of Baily's metal for distribution to foreign governments, only two are exactly standard at 62° F.,—"Bronze 19" and "Bronze 28." "Bronze 28" is kept at the Royal Observatory as an accessible representation of the national standard.

All the other copies have the temperature, at which they are standard, marked upon them.

In 1856 "Bronze Bar No. 11" was presented by the British Board of Trade to the United States; at that time it was declared to be standard at 61.79° F. According to recent comparisons this bar is *now* .000088 inch shorter than the Imperial Yard No. 1.

In reproducing a standard bar, whether for reference, or as a *working* standard, line or end measure, or both, care must necessarily be taken to know *positively* that the surface, upon which the lines are ruled, is a plane surface, in other words, to avoid the slightest amount of flexure, which would obviously vary the distance between the lines, especially when these lines are upon the outer surface of the bar, and hence, in supporting a bar, the points of support have been found by Sir George Airy to be the distance apart represented by the formula:

$$\frac{\text{Length of bar}}{\sqrt{n^2 - 1}}$$

"n" being the number of supports. When there are two supports this formula gives 10.39 inches for the distance between the supports and the centre of the bar in the case of the yard bars, and 28.87 centimeters in the case of the *meter* bars.

Placing the gold plugs at the bottom of the wells, sunk half way into the bronze bar, was intended to overcome the difficulty of flexure, as the lines would then be at the best plane of variation caused by flexure, still, by placing the bar upon supports in such a way as to neutralize this tendency of bending, and having the surface carefully worked to a plane under a microscope of a high power before the lines are ruled, this difficulty is removed, if the lines which

are subsequently traced remain in focus throughout the entire length of the bar.

Professor Rogers's method of using a mirror surface of mercury as a reference plane for working the guiding surface or "ways," on which the microscope plate slides, is that adopted, and the use of a microscope of high power gives a very accurate result, the perfect focus obtained along the entire length of the mercury trough proving conclusively that the microscope plate moves in a true plane.

In the new comparator constructed by the Pratt & Whitney Company, under the direction and from plans suggested by Professor Rogers, the means for overcoming objections and difficulties arising from errors due both to horizontal and vertical curvature, deflection, etc., are fully provided for.

The plan adopted for securing accurate sliding motion of the microscope plate is perfect *line-bearing*, and the uniform pressure is due to gravity simply, and the bearing surfaces, or guides, are such that errors due to imperfect straight-line action may easily be remedied.

The flexure of the guides is also provided for by supports placed at about one-quarter the distance from each end of the guide-bars, which are heavy hardened-steel tubes, ground perfectly true and parallel, using counter-weights to overcome the flexure arising from their own weight and the weight of the moving microscope plate.

The bars used as standards by the Pratt & Whitney Company comprise:

I. A bronze bar of Baily's metal, having lines ruled on sunken gold plugs. It is a yard measure, with subdivisions into feet only. This bar is designated in the official report as "P. & W_1."

II. A bar of Baily's metal, identical in composition, and having the same section as "P. & W_1." It is 42 inches long, and has lines ruled on the surfaces of plugs carefully inserted, made of an alloy of platinum and iridium; these plugs are $\frac{3}{32}$ of an inch in diameter, and are polished to a mirror surface. This bar has lines representing the yard at 62° Fahrenheit, with subdivisions to feet and inches, and the meter at 62° Fahrenheit.

The alloy of platinum and iridium gives clear smooth lines when ruled with the finest diamond edge, and in order to prevent accidental defacing, or injury from any cause, the lines are covered with disks of glass $\frac{1}{100}$ of an inch thick. This bar is denominated, in the report, "P. & W_2."

III. A yard and meter bar, of hardened steel, on the upper pol-

ished surface of which are ruled lines corresponding to those upon "P. & W_2," but having, in addition, *end* measure for the yard at 62° F., and for the meter at 32° F.

The neutral points of support, *i. e.*, those of least flexure, are left as "spots" on the under side of this bar, so as to avoid mistakes due this cause when in use. This bar is marked "P. & W_3."

IV. A steel yard and meter bar, untempered, but having the same form as the preceding, the only difference being that the yard and its subdivisions, and also those of the meter, are ruled upon the mirror-surfaces of hardened steel plugs, the *end* measure for the yard and meter also being determined by plugs of the same material, fitted in each end, and protected from injury by an extension of the upper surface. This bar is designated "P. & W_4."

After the preparation of these bars at the works of the Pratt & Whitney Company, they were forwarded to Professor Rogers, at Cambridge, for the purpose of receiving the graduations. An additional bronze bar, the exact duplicate of "P. & W_2," was also sent, on which a provisional transfer of the yard from the steel bar in his possession was made, after applying the reduction to the Imperial Yard given by Mr. Chaney, the Warden of the Imperial Standards. This provisional bar was then forwarded to Washington, Professor Hilgard having kindly consented to compare it with "Bronze 11."

According to the report of Professor Hilgard, this yard is .000025 inch shorter than "Bronze 11."

The yards traced upon "P. & W_1" and "P. & W_2" were obtained from this provisional yard. They were then sent to Washington for final comparison with "Bronze 11."

According to the official report of Professor Hilgard, after allowing for the known relation between "Bronze 11" and the Imperial Yard, "P. & W_1" is .000053 inch longer than the Imperial Yard, and "P. & W_2" is .000036 inch shorter than this unit.

The yards and meters upon the steel bars were derived from "P. & W_1" and "P. & W_2" after the reduction of the relative co-efficient of expansion between bronze and steel.

V. A hardened-steel six-inch bar, one-half inch square in section, having upon its upper polished surface, lines ruled four separate inches, also lines representing—counting from the end of the second inch—the lengths corresponding to the *bottom* diameters or "tap-sizes" of the United States or Franklin Institute standard screw-threads, from a quarter-inch to four inches.

Besides this band of irregular spaces are ruled two inches in sixteenths and two inches in twentieths of an inch; also, a band of two inches at twenty-five hundred per inch, the latter being used in the investigation of the irregular lengths or "tap-sizes."

This six-inch bar was ruled at the American Watch Factory, Waltham, upon a dividing engine constructed by the Watch Company, from designs furnished by Professor Rogers, for his use in producing standards of length. The accuracy of the settings, and the remarkable freedom from error found, upon a rigid investigation subsequently made, prove the excellence of the workmanship in the construction of the machine.

It having been found necessary to regraduate this bar to accommodate the sizes for larger diameter thread-gauges than was at first intended, a complete new series of irregular lengths was made, the new lines being ruled as nearly .001 inch apart as it was possible to set the diamond.

Upon comparing results the variation was found to be less than .0005 inch from the constant interval between the new and the old lines.

When it is considered that nearly four weeks had elapsed since the original ruling was done, and that the same settings were used, the extreme accuracy of the screw of this machine may be appreciated.

The lines upon this bar are less than .000066 inch in width, the cross-line in the eye-piece of the microscope being usually brought to cover either the *edge*, or the *middle* of the furrow made by the diamond cutter.

End-measures of hardened steel of the same brand as the hardened screw gauges have been made from a quarter of an inch to four inches in length, varying by sixteenths, and corresponding to the lines on the six-inch bar. With this bar, the problem of maintaining uniform sizes in actual use is a very simple one.

The practical difficulties met with in using microscopes of high power, where extreme accuracy is necessary, render the use of any form of reflector very objectionable, as the reflected image is often distorted.

In the use of Tolles's illuminator, in which a prism is inserted within the objective of the microscope, this difficulty is obviated, giving sharply defined lines upon opaque surfaces, such as steel or bronze, and especially upon the plugs of platinum and iridium.

The two objectives used upon the comparator belonging to the

Pratt & Whitney Company were furnished to order by Mr. R. B. Tolles, of Boston, and both have this form of illuminator attached.

Referring back to the second natural unit for establishing a standard of length,—that of using the ten-millionth part of the earth's circumference,—the result of the labors of a commission appointed by the French government was four iron bars, the ends carefully ground until exactly comparable with each other, and each having the required length. One of these original bars, bearing the stamp of the commission, is now in the possession of the United States Coast Survey. From these bars the present meter of the archives was constructed.

Of the third and last unit proposed,—that of a wave-length of given refrangibility,—it is doubtful whether this as a unit can ever be successfully adopted for general use; since the measurements of wave-lengths for an entire meter vary so much as to make the total length of a yard or meter known to a far less degree of accuracy than can be assigned to the comparison of different standards.

In conclusion, then, whenever the yard with its subdivisions is adopted as the measure of length, the unit to which all measures must be referred, is the bronze bar deposited in the "Strong Room" of Old Palace Yard, London, and known as the "Imperial Yard, No. 1."

I quote Professor Rogers's statement regarding the existing metric standards:

> "Wherever the metric system has been adopted, either by legal enactment or by actual use in the absence of definite legislation, the platinum end-measure meter deposited in the archives of Paris, is the only ultimate standard of reference."

The method adopted for the accurate subdivision of the yard and meter upon the comparator of Professor Rogers's design, is to compare the arbitrary or trial divisions first, by finding their relation to each other, with a fixed distance between immovable stops, and noting the time-worn axiom, that "things equal to the same thing are equal to each other." The yard or metre being correct in total length, the differences from the mean form an algebraic sum, the value of which is evidently equal to zero.

The micrometers for use in the standard work by the Pratt & Whitney Company were furnished by James Queen & Co., Phila-

delphia, and bear the name of "J. Zentmayer" as a guarantee of their excellence.

The coefficients of expansion of both the bronze and steel bars, tempered and untempered, in the possession of the Company, have been carefully determined by Professor Rogers, the investigation covering a period of nearly two hundred days, under every possible condition of temperature, in air, and immersed in water, and the changes due to differences of shape and mass have been carefully noted. The changes of temperature of the bar must affect the mass throughout uniformly, and ordinarily from six to twelve hours is necessary to allow these changes to be effected before the comparison is made, the temperature meanwhile having been kept as nearly constant as possible.

I may add, in conclusion, that the standards in the possession of, and used by Professor Rogers, comprise:

(*a.*) A nickel-plated hardened steel bar, the lines upon the nickel surface having been compared directly with the Imperial Yard by Mr. Chaney, Warden of the Standards at London, during the visit of Professor Rogers in England.

(*b.*) An end-measure Coast Survey yard kindly loaned by the Stevens Institute of Technology, of Hoboken, N. J.

The Coast Survey yard has been compared directly with the "working" yard of the Exchequer by Mr. Chaney.

(*c.*) A meter, line-measure, the lines traced upon the middle surface of an X-shaped copper bar, of small mass, this form having been adopted by the International Bureau of Weights and Measures.

This bar was traced for Professor Rogers during his visit at Paris, in February, 1880, by M. Tresca, and is signed by him.

(*d.*) A steel end-measure metre, made by M. Froment, of Paris, and declared to be 8.43 mikrons (about .00033 inch) longer than the metre of the archives.

As was mentioned at the beginning of this paper, the intention is simply to report progress, and to show how far the "vital" part of this subject of standard measurements has been carried.

That part of the work which may be regarded as completed is the determination of the entire length of the yard as represented by the bars "P. & W_1" and "P. and W_2," since according to the report of Professor Hilgard, the mean of the two yards differs from the Imperial Yard by a quantity less than the *certainty* with which such comparisons can be made, viz., .00001 inch.

All the work so far described has been done with a comparator

having some faults in construction, and, although the errors due to imperfections have been allowed for, still it has been deemed wise to defer the publication of the full report of Professor Rogers until all the other measures have been verified by observations with the new comparator. It is confidently expected, however, that no errors of appreciable magnitude will be found in the working six-inch bar, upon which all the standard gauges depend.

In concluding this paper it is a pleasure to add, that through the generous action and untiring energy of James E. Denton, Mechanical Engineer, of the Stevens Institute, much has been done to aid the efforts of the Pratt & Whitney Company in establishing a standard of length upon a practical basis, and also to express appreciation of the endeavors of the Master Car Builders' Association to bring about uniformity of size in bolts and nuts for car-construction in the United States.

Much is also due to the lively interest and hearty co-operation of Mr. M. N. Forney, chairman of the committee appointed by them to consider the matter, and who fully realizes the benefits arising from a general adoption of a uniform system of standard size gauges.

DISCUSSION.

The President: I presume that all agree with me that the enterprise of the Pratt & Whitney Company, and the beautiful work done by Professor Rogers and Mr. Bond, are equally admirable. I think it is the first time in which accurate, scientifically determined, standard measurements have ever been introduced into commercial work of the character which most of our shops are doing. I think it is a start in a new direction, and it shows peculiar enterprise and a very unusual appreciation of the value of this sort of research, and of the importance of beginning at a base line accurately and fully determined.

Mr. Smith: I want to ask one question of Mr. Bond in regard to a point in his valuable paper; that is, whether those careful and extended measurements were made to coincide at all with the measurements of the Betts Machine Company, who are the only other people I know of in the country that have carefully-made sets of standard gauges for the market.

Mr. Bond: I believe in the official report which will appear that this will probably be settled by comparison with the two reports. Of course the exact figures it would be hardly fair to give,

and they are not known to a certainty now,—that is, to exactness. So that it will be best to postpone that until after the report, which will probably appear at the next meeting.

Mr. Smith: I only spoke of it because some of us have purchased gauges, and we do not want to feel that there are two standards.

Mr. Pratt: I wish to say, Mr. President, that this investigation has been going on without much regard to what other people have done. Upon examining other measuring devices and standard gauges we found that they did not agree with ours or with each other, and of course we had no other resort but to go to what may be considered the bottom of the thing, and the paper has shown how far we have gone in it. As Mr. Bond says, we hope to have, before we introduce them, the indorsement of the very best engineers of the country. We shall do nothing without it. We expect it will be indorsed by them. They will be the judges as to whether we are right or wrong. They must see the apparatus and must know all about it. Our assertion is not sufficient; we do not make an assertion. We intend that the best talent of the country shall judge of it. We hope to finish it within six months. I would say in regard to Mr. Smith's proposition, that I wish this question had been brought up some time ago. It might have saved our company a great deal of expense and hard labor. We have gone thus far in this thing at great cost. Within two years we, I think, must have spent on this standard-measurement business, in connection with the gauges which it grew out of, not less than fifteen thousand dollars, and how much more I cannot tell. It came about by our company being applied to for a set of United States standard-thread gauges, and we could not satisfy ourselves with our own or any standard we could find. We applied to the best makers, and, as the President well knows, the foot-piece that we had made by one of the best makers in the country—if not the best—was submitted to the Stevens Institute of Technology, and they, in connection with Professor Rogers, of Cambridge, tested this piece, and did not agree with its maker as to its value by an average of .0007 inch; one party declaring that it was so much too long, while of course the party that made it maintaining that it was right. We could not proceed with confidence, and consequently, as I said, we have gone, as we think, as near hard-pan as it is possible to go. Professor Rogers has done much to help us in this mat-

ter. We should like to have some one else take the expense and the trouble; but we do not propose to back out of it; we shall show something, I think, in six months that cannot but gratify; and, with the indorsement of such men as we hope to have, will be perfectly satisfactory, I think. At least if we do not have it, we shall not introduce it.

MR. SMITH: What you have said to-day will give us confidence in it. I only wanted to know how near the two things were working together.

THE PRESIDENT: I have no doubt that all agree that the thanks of the community of engineers are due to the Pratt & Whitney Company for the thorough way in which they are doing that work, and for the establishment of a standard.

XXVII.

FORMULA FOR THE HORSE POWER OF LEATHER BELTS.

BY A. F. NAGLE, PROVIDENCE, R. I.

FOR laced belt:

$$HP. = (.55 - .00002157\ V^2)\ V\ t\ w\ C$$

For riveted belt:

$$HP = (1. - .00002157\ V^2)\ V\ t\ w\ C$$

$C = (1 - 10.^{-0.00758 f a})$

$@$ = Degrees of belt contact.

w = Width of belt in inches.

t = Thickness of belt in inches.

f = Coefficient of friction = .42.

V = Velocity of belt in feet per second.

V becomes a maximum at 92 feet per second for laced belts.

V becomes a maximum at 125 feet per second for riveted belts.

NOTE.—These formulas are upon the basis of a maximum stress of $66\frac{2}{3}$ pounds per inch of width for laced belt, and $121\frac{1}{3}$ pounds per inch of width for riveted belt, each $\frac{7}{32}$ of an inch thick, being one-third of the breaking strength of the belt. Weight of leather taken at 55 pounds per cubic foot.

In my present practice with mill machinery, the question of the power of belts is of constant occurrence; and, in turning to the books upon the subject, I did not find satisfactory answers. For instance, in Samuel Webber's *Manual of Power*, page 98, are given no less than five formulas, all of substantially the same

form as the one given by Mr. Webber himself, for single leather belts, viz.:

$$w = \frac{HP \times 5500}{\text{velocity} \times \text{contact in feet}}.$$

This formula, expressed in terms of the diameter and revolutions, becomes:

$$w = \frac{HP \times 5500 \times 360^\circ}{D^2 \times Rev. \times \pi^2 \times @},$$

@ being the degrees of contact; the other notations are well understood. This makes the width vary inversely as the square of the diameter; and also inversely as the simple multiple of the arc of contact, and takes no cognizance of the centrifugal force.

All mathematical investigations indicate that the width varies inversely as the diameter instead of the square; and the adhesion or friction varies as a logarithmic function of the arc of contact instead of a simple multiple.

I turned, therefore, to a recent book by Mr. John H. Cooper, on the *Use of Belting*, wherein I found a large and valuable collection of matter on the subject, but no attempt was made to assimilate the same.

I presume that all that is necessary for a complete solution of this problem can be found within those pages, except some mathematical investigations for which we can refer to our text-books.

There we have General Morin's experiments, and later, those of Mr. Henry R. Towne. From this mass of data I have constructed the new formula, which, I think, covers all conditions of belt power, which are its

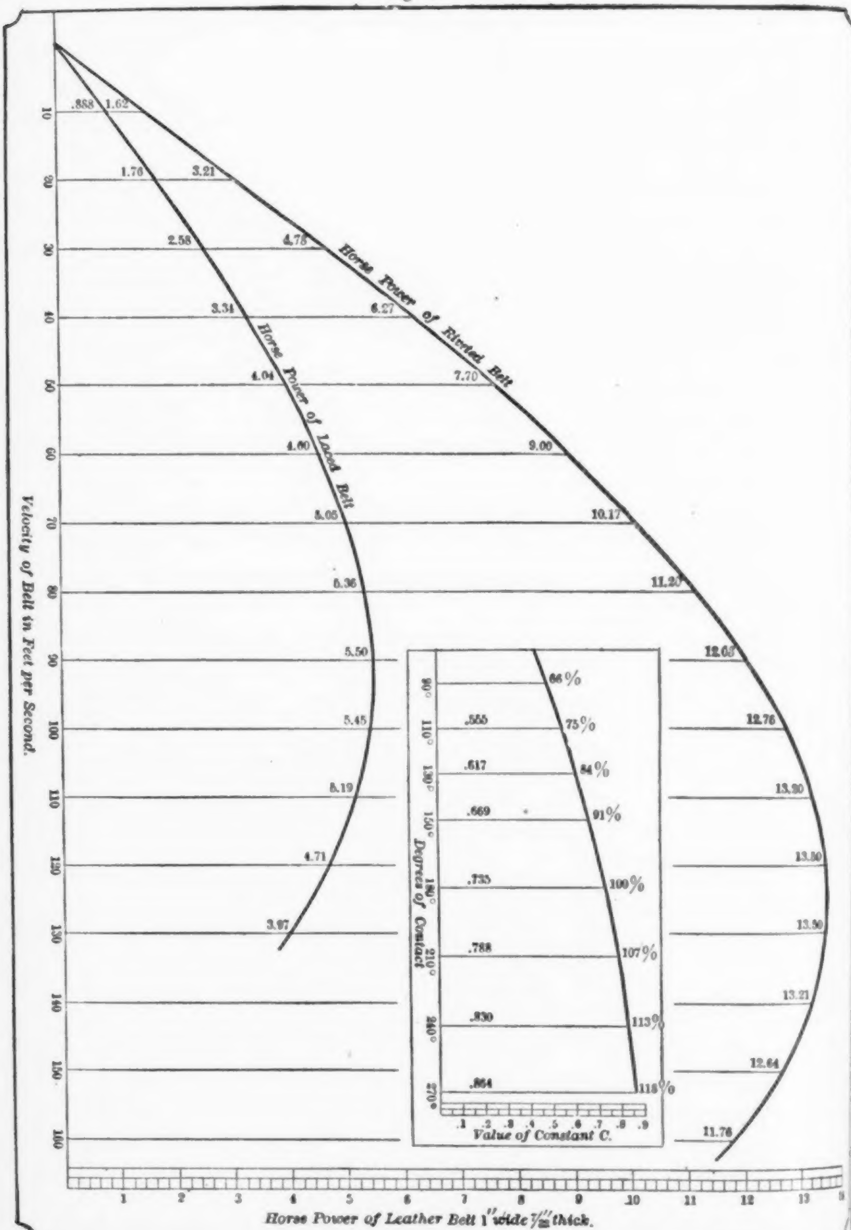
1. Strength and tension.
2. Coefficient of friction.
3. Degree of contact.
4. Velocity.

1. STRENGTH AND TENSION.

Mr. Towne's experiments upon leather belts 3″ wide, and $\frac{7}{32}$″ thick, are generally accepted among engineers as furnishing reliable data for calculations of other sizes. Reducing to the unit of measure of one inch width, he found the ultimate strength to be:

At the lacings	210 pounds	100	55	31
At the rivets	382 pounds	182	100	56
At the solid part	675 pounds	321	177	100

Fig. 23.



Graphic Illustration of the Formula as applied to a belt one inch wide and seven thirty-seconds thick

He determined upon 200 pounds as the ultimate strength at the lacings, and adopted a coefficient of safety of 3, thus giving the maximum working tension of $66\frac{2}{3}$ pounds per inch of width, at $\frac{7}{32}''$ thickness.

For a riveted joint this would be increased eighty-two per cent., or to 121.33 pounds.

I shall use this same working tension in my subsequent calculations.

The strength of leather, like that of other materials, is proportional to its area of section. Denoting the thickness by t, and the working tension by S, the above working tension will be expressed as follows:

For laced joints,

$$S = 66.667 \times \tfrac{32}{7} \times t = 305t;$$

for riveted joints,

$$S = 1.82 \times 305t = 555t.$$

2. COEFFICIENT OF FRICTION.

The coefficient of friction f of leather upon iron, General Morin found to be .56 when dry, .36 when wet, .23 when greasy, .15 when oily. Mr. Towne's experiments were made with new, partially worn, and well-worn belts, upon iron pulleys, and under such conditions as ordinarily prevail in the workshop. He finally determined upon $f=.42$ as a coefficient that could be safely depended upon. It is also a well-established fact that this coefficient is independent of the pressure and surface of contact. In Mr. Towne's experiments, the pressure varied as 20 to 1 on pulleys from 12″ to 41″ in diameter. In General Morin's the pulleys varied from $4\frac{3}{8}''$ to 24″ in diameter, and the width of the pulley was reduced in one case to sixty per cent. of the width of the belt.

3. DEGREES OF CONTACT.

The law according to which a belt transmits force (F) by virtue of its frictional resistance and degrees of contact, is shown by Professor Rankine to be expressed by the formula:

$$F = S\,(1 - 10^{-0.00758 f @}), \qquad (1)$$

in which

F = force transmitted,
S = working tension,
f = coefficient of friction,
@ = degree of contact.

If we call the expression in the parenthesis C, substitute the value of $f = .42$, and solve for various values of @, C becomes:

TABLE 1.

@	90°	110°	130°	150°	180°	210°	240°	270°
C	.485	.555	.617	.669	.735	.788	.830	.864
°/₀	.66	.75	.84	.91	1.00	1.07	1.13	1.18

A diagram accompanies this paper, graphically illustrating this table.

4. VELOCITY.

The velocity of a belt, multiplied by the force with which it moves, is an expression of its power. When this velocity is great, the centrifugal tension engendered thereby must be considered. Professor Rankine expresses it as equivalent to "the weight of a piece of the belt whose length is twice the height from which a body must fall in order to acquire the velocity of the belt." By formula, the centrifugal tension is

$$T = \frac{WV^2}{g}, \qquad (2)$$

where W = weight of a unit of length of the belt, and if the cubic foot weighs 55 pounds, the unit of length of one foot an inch wide, and t inches thick, would weigh

$$W = \frac{55t}{144} = .382t \text{ pounds.}$$

V = velocity in feet per second,
g = velocity produced by gravity in a second = 32.2 feet.

With these values substituted in equation (2) we have

$$T = \frac{.382t \times V^2}{322} \text{ or } .0118615t\,V^2. \qquad (3)$$

This centrifugal tension acts upon the belt regardless of any tension produced by any power transmitted, and, of course, by so much diminishes its capacity for transmitting useful work. In other words, if S is the maximum stress to be applied to the belt, whatever centrifugal tension there may exist, must reduce S by so much for useful work. Or, available stress becomes $S—T$, or substituting the value of T, becomes

$$S. - 018615\,t\,V^2$$

and equation (1) becomes

$$F = (S - .0118615\,t\,V^2)C. \qquad (4)$$

The horse-power is finally obtained by multiplying the net effective force per second, and width of belt in inches w, and dividing by 550 (the foot pounds of a H P per second), or

$$HP = \frac{FVw}{550} \tag{5}$$

for laced belts; or by substitution

$$HP = (305\,t - .0118615\,t\,V^2) \times \frac{V \times w}{550} \times C,$$

or,

$$HP = (.55 - .00002157\,t\,V^2)\,V\,w\,t\,C, \tag{6}$$

and for riveted joints

$$HP = (1.00 - .00002157\,V^2)\,V\,w\,t\,C. \tag{7}$$

NOTE.—Equation (6) becomes a maximum when $V = 92.57$ feet per second $= 5554$ feet per minute, and (7) when $V = 125$ feet per second $= 7500$ per minute.

This formula, I believe, contains all the elements that enter into the problem, and I repeat the factors entering into it, so that it may be clearly understood how it is made up; and if any special reasons should exist for changing those factors it may be done.

Weight of leather = 55 pounds per cubic foot.

Stress upon laced joints = $66\frac{2}{3}$ pounds per inch width, at $\frac{7}{32}''$ thickness. Stress upon riveted joints 1.82 times as much, which, in either case, is one-third of the ultimate strength of said joint. Coefficient of friction of leather on iron = .42. Value of C for arc of contact as may be taken from Table 1.

I append the following list of belts in use, and the width they should be, by either Mr. Webber's rule or mine.

The data are taken principally from Mr. Cooper's book, the *Journal of the Franklin Institute*, and some from my own experience. The data are not always as complete as they should be, but I have approximated as nearly to the truth as I could. I have considered the belts as *laced*, and when no thickness was given, such as the circumstances would warrant.

Horse Power.	Velocity in Feet per Minute.	Diameter of Small Pulley.	Pull of Belt per Inch of Width—Pounds.	Width of Belt in Use.	Width by Webber's Rule.	Width by Nagle's Rule.	Thickness.
375	5600	5′—0″	98	24″	22″	34″	Double.
250	3080	7′—0″	58	48″	50″	28″	4-ply.
220	2451	3′—6″	135	22″	98″	31″	Single.
175	3179	6′—0″	93	19½″	15¾″	25″	Double.
175	3629	9′—7⅛″	55	29″	15″	22″	"
130	2117	5′—10″	113	18″	18″	22″	"
125	3490	7′—0″	82	14½″	8″	17″	"
90	2860	5′—0″	87	12″	10″	15″	"
77	2268	5′—0″	77	14½″	12″	12″	"
45	2000	4′—0″	37	20″	21″	15″	Single.
49	2111	6′—0″	24	18″	14″	18″	"
43	1800	5′—0″	44	18″	20″	14″	"
40	2000	6′—0″	37	18″	14″	13″	"
41	1809	5′—0″	42	17½″	12″	16″	"
18	850	0′—22″	116	6″	19″	8″	Double.
8	942	2′—6″	40	7″	12″	8″	Single.

DISCUSSION.

Mr. Emery: I will say one word here, which applies to this particular subject. I did not understand the gentleman's reading of the first formula, C = one times, the number corresponding to .00758. Generally that would be read, I suppose, E to the logarithm, .00758 times *f*. That is the way it is generally read, I should think, from the notation.

Mr. Nagle: The empirical formulas that are given for belts, mentioning the surface of contact, where the surface of contact is a factor, I apprehend, are introduced from the fact that the high speeds are usually obtained by small pulleys; that is, a large pulley communicating the motion to a small pulley, thus of course losing some of its contact on the small pulley. The empirical formula is an approximation to provide for the angle of contact and the centrifugal force. In that equation both are taken cognizance of mathematically, and coefficients are substituted for them, so that I think that it meets all practical cases.

Mr. Alden: I would like to inquire whether the practical results of the experiment confirm the formula, or otherwise; whether it was found that the formula agreed with the practical results, or disagreed largely?

Mr. Nagle: There is a very wide diversity in the results of these experiments. In Mr. Cooper's book on belting there are some experiments made in a similar manner to those of Mr.

Webber, and those experiments practically confirm the theory. I can pick out isolated cases from these experiments that confirm the theory; others will not. I have not had time enough to examine it as closely as I will by and by; but I see occasionally a figure that will conform to the theory. You must remember that they are always making so many experiments, producing diverging results, that it is very difficult to draw a conclusion from any one of them.

Mr. Emery: Where did I understand the gentleman got the formula he speaks of?

The President: That is the accepted formula. You will find it in Rankine.

Mr. Emery: I did not understand the explanation of the formula as given. The equation is familiar. The strain on the belt is gradually reduced by friction from the beginning to the end of the arc of contact. The formula arises from the summation of a decreasing series and is derived from the equation of an equilateral hyperbola. The friction is proportioned to the total change of angle and not to the surface. A similar equation will be found developed by Professor Morton, in the *Journal of the Franklin Institute*, some years ago, in discussing Mr. Towne's experiments, which I quoted in my Centennial Report.

XXVIII.

AN IMPROVED MERCURY COLUMN.

BY D. N. MELVIN, STATEN ISLAND, N. Y.

Every one who is in any way familiar with the generation and use of steam at high pressure, knows well the unreliable nature of the ordinary spring pressure gauge or manometer.

On this account, resource is sometimes had to the open mercury column, where the pressure is not so high as to entail the erection of a very high siphon.

On many of our steamers the mercury column is still used in conjunction with the spring gauge, principally to check the accuracy of the latter. Even the so-called test-gauges, manufactured specially as standards, require to be tested periodically with a mercury column; and for this purpose, numbers of firms have,

at considerable expense, erected open mercury columns for high pressures.

The object of this paper is to show how the open mercury column can be brought into more general use at a comparatively small cost, as a standard test-gauge where spring gauges are in use, without the necessity of erecting a high unwieldy siphon, or in fact making any special arrangements whatever, even for the highest pressures.

About five years ago the writer, being very much annoyed by the discrepancy between a number of spring gauges he had in use, determined to erect an open mercury column, but the great height required, and the unwieldy nature of the instrument, induced him to seek for some method of bringing the whole apparatus within moderate bounds.

That well-known artifice employed by Fahrenheit for reducing the height of the barometer seemed to offer the means, and on investigation it was found that this artifice had already been employed for this purpose under the name of the "Differential Manometer," and it is described in *Deschanel's Natural Philosophy*, and also in *Weisbach's Mechanics of Engineering*, Vol. 2.

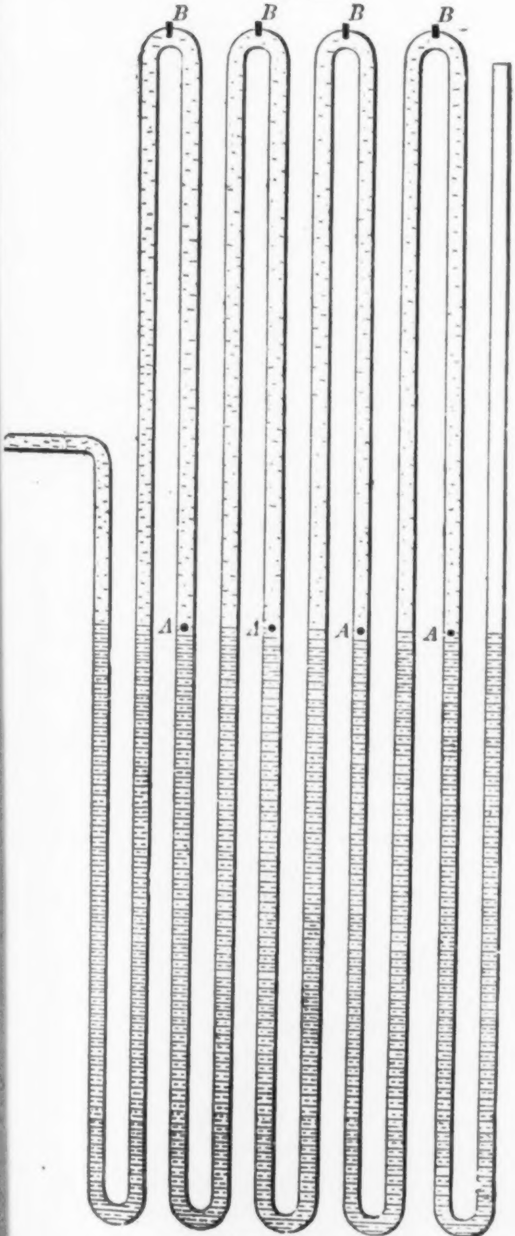
Although several attempts have been made by parties to introduce this device, yet it has not come into general use; probably because of the difficulties attendant on filling the tubes, and also on the expansion by increase of temperature becoming more apparent than in a single siphon, the whole elongation of the contents being transmitted to the outer ends; and as the readings have been taken from the elevation of the mercury in the last tube, they would necessarily be very inaccurate. By simply constructing the scale to read the difference of level in the two outer tubes, the effect of expansion by increase of temperature in this way is counteracted, leaving only the actual decrease of specific weight to be taken into account, as in a single siphon gauge.

On calculating the effect of different liquids used in conjunction with mercury, it became apparent that the whole of the effects of expansion could be eliminated, and the instrument rendered accurate at all temperatures within ordinary range.

The diagram represents a gauge with five siphons, the siphons being spread out on the plane of the paper instead of in a square nest; and the engraving shows the same gauge as in use.

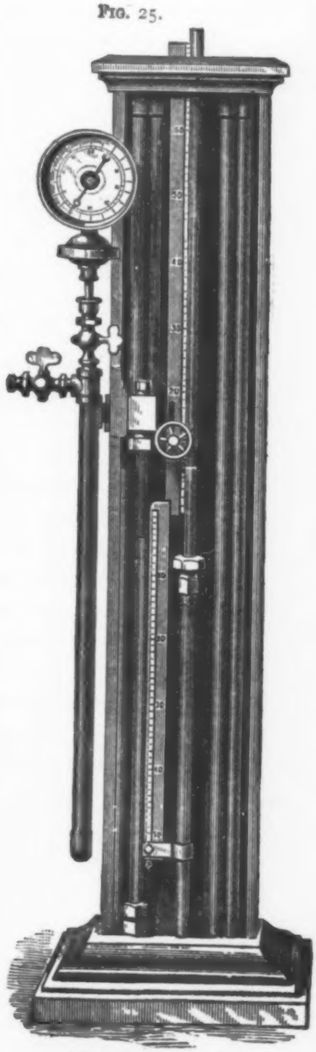
The lower parts of the tubes are filled with mercury up to the holes *A*, *A*, *A*, *A*. These holes are then plugged with taper

Fig. 24.



SECTIONS OF SIPHONS.

Fig. 25.



MERCURY GAUGE.

screw plugs, and made perfectly tight; the top parts of the tubes are then filled with some other liquid of known specific gravity and coefficient of expansion, through the holes marked *B*, *B*, *B*, *B*. These holes are in turn plugged, and the gauge is ready for use.

Upon the application of pressure to the first column, it is depressed a certain amount, the pressure is transmitted through the second liquid in the upper parts of the siphons, and all the other columns are depressed the same amount, provided the tubes are all of uniform bore.

If the second liquid merely transmitted the pressure without of itself possessing weight, the height of a single column of mercury, equivalent to the pressure, would be the sum of the differences of level in each of the siphons; but the weight of the column of the second liquid is added to the pressure, so that the sum of the differences is too great by this amount; therefore this has to be deducted, and the formula stands as given in the text-books: An equivalent height of single mercury column is expressed by

$$nd\left(1-\frac{1}{13.59}\right)$$

where n is the number of siphons, and d the difference of level; 13.596 being the specific gravity of mercury.

Water is here used as the second liquid, and consequently unity is the numerator of the fraction; and if any other liquid is used, its specific gravity must be substituted in the numerator.

Any increase of temperature expands the contents of the tubes; this expansion is transmitted to the outer ends of the siphons, and if the bore of the tubes is uniform, the surfaces of the mercury in the two outer tubes are unequally elevated above their original position, and the difference of height between the surfaces of mercury, or the height of the column, becomes increased with every increment of temperature; but as the second liquid also becomes lighter, the total pressure on the mercury becomes less.

The correction for temperature then depends on the relative densities of the two liquids, as well as on the actual expansion of mercury. The above formula will give the equivalent height of a mercury column at any temperature, if the specific gravities or densities of the two liquids at the desired temperature are substituted in the minus quantity. If this ratio, or the relative densities of the two liquids remained the same as the temperature

increased, then the correction would be the same as in a column of mercury of the same height; but, if this ratio increased with every increment of temperature as the specific gravity of mercury decreased, or, in other words, if the second liquid were to lose as much specific weight as mercury did by increase of temperature, then the actual height of the column, *i. e.*, the difference of level between the surfaces of mercury, would remain the same, and these surfaces would be equally elevated above their original position, whatever variations of temperature might take place.

To accomplish this it is only necessary to select for a second liquid, a substance whose specific gravity, multiplied into its coefficient of expansion, is equal to the specific gravity of mercury multiplied into its coefficient of expansion; or whose density will decrease with each increment of temperature at the same rates as the density of mercury decreases.

This is shown in the accompanying table, in which the variation of a thirty-inch column on a five siphon gauge is shown, both for water and tetrachloride of tin used as second liquids. I have chosen tetrachloride of tin to illustrate with, as its decrease of density, with increase of temperature, is rather greater than that of mercury, causing the column actually to shorten as the temperature rises, as will be seen by inspection of Column 3. From Columns 5 and 7 it will be seen that the decrease in specific gravity of mercury and tetrachloride of tin is nearly the same, and if they had been exactly alike, the height of the column, or the reading height of the gauge, would have remained constant as the temperature varied.

1	2	3	4	5	6	7
Temperature. Cent.	Height of Column, with Water as Second Liquid.	Height of Column, with Chloride of Tin as Second Liquid.	Specific Gravity of Mercury.	Difference of Spg. of Mercury.	Specific Gravity of Chloride of Tin.	Difference of Spg. of Chloride of Tin.
0°	30.00000	30.00000	13.59600	.0244	2.2671	.0256
10°	30.05420	29.99657	13.57167	.0243	2.2415	.0255
20°	30.11050	29.99337	13.54736	.0233	2.2160	.0254
30°	30.16050	29.99010	13.52405	.0251	2.1906	.0258
40°	30.21465	29.98642	13.49895		2.1648	

The specific gravities of mercury are taken from the tables of Regnault, and those of tetrachloride of tin are calculated by the aid of the formula of Pierre.

The accompanying engraving was taken from a gauge the

writer has had in constant use for the last four years. The second liquid used was a mixture of salt water and glycerin, and it has answered the purpose admirably; and being compact, it could easily be kept at a constant temperature.

Dr. Andrews, of Queen's College, Belfast, in a paper read before the Royal Society, says: "It is with regret I am still obliged to give the pressures in atmospheres, as indicated by an air, or hydrogen manometer, without attempting for the present to apply the corrections required to reduce them to true pressures. The only satisfactory method of obtaining these corrections would be to compare the indications of the manometer with those of a column of mercury of the required length. . . . For pressures corresponding to 500 atmospheres, at which I have no difficulty in working with my apparatus, a mercurial column of the enormous height of 1250 feet would be required. Although the mechanical difficulties, in the construction of a long tube for this purpose, are, perhaps not insuperable, it could only be mounted in front of some rare mountain escarpment, where it would be practically impossible to conduct a long series of delicate experiments."

Here, then, is an instrument which would accomplish all this; and if no greater height could be allowed, one hundred siphons of fifteen feet each would test up to five hundred atmospheres easily.

XXIX.

THE FIRST ROLLING-MILL IN AMERICA.

BY WILLIAM H. HARRISON, BRAINTREE, MASS.

THE accompanying drawings are plan and elevation of the machinery of a rolling-mill, built at Middleboro, Massachusetts, for Peter Oliver, one of the Crown Judges in the Province, and a brother of Andrew Oliver, the Lieutenant-Governor—in the year 1751. They possess no scientific interest, but it is perhaps proper for the representatives of mechanical science in this country, at this, their first meeting in New England, to put on record some account of the work of the mechanics of this section in the olden time.

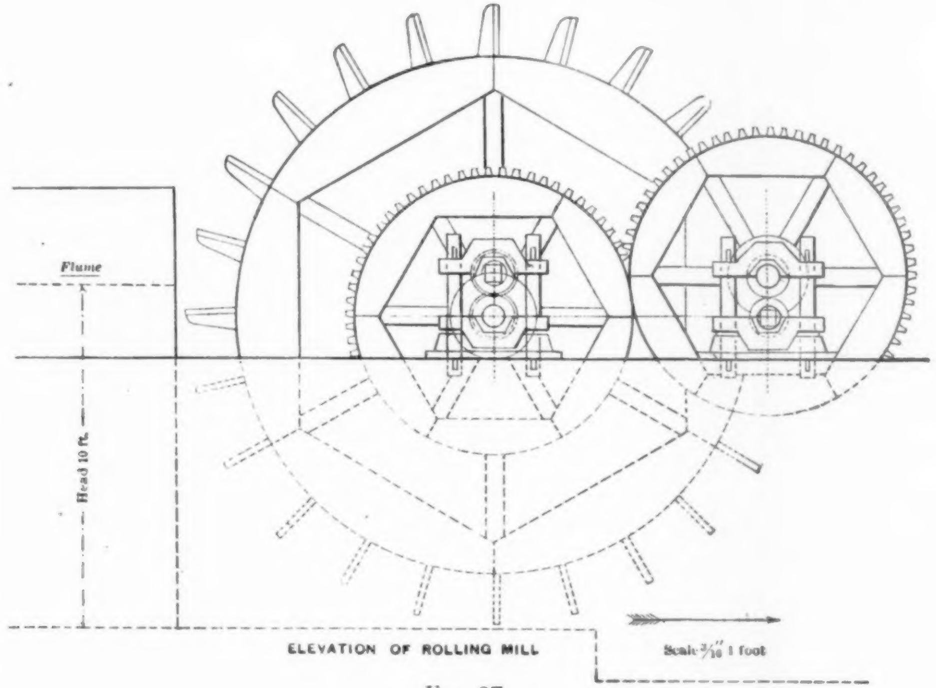


Fig. 27.

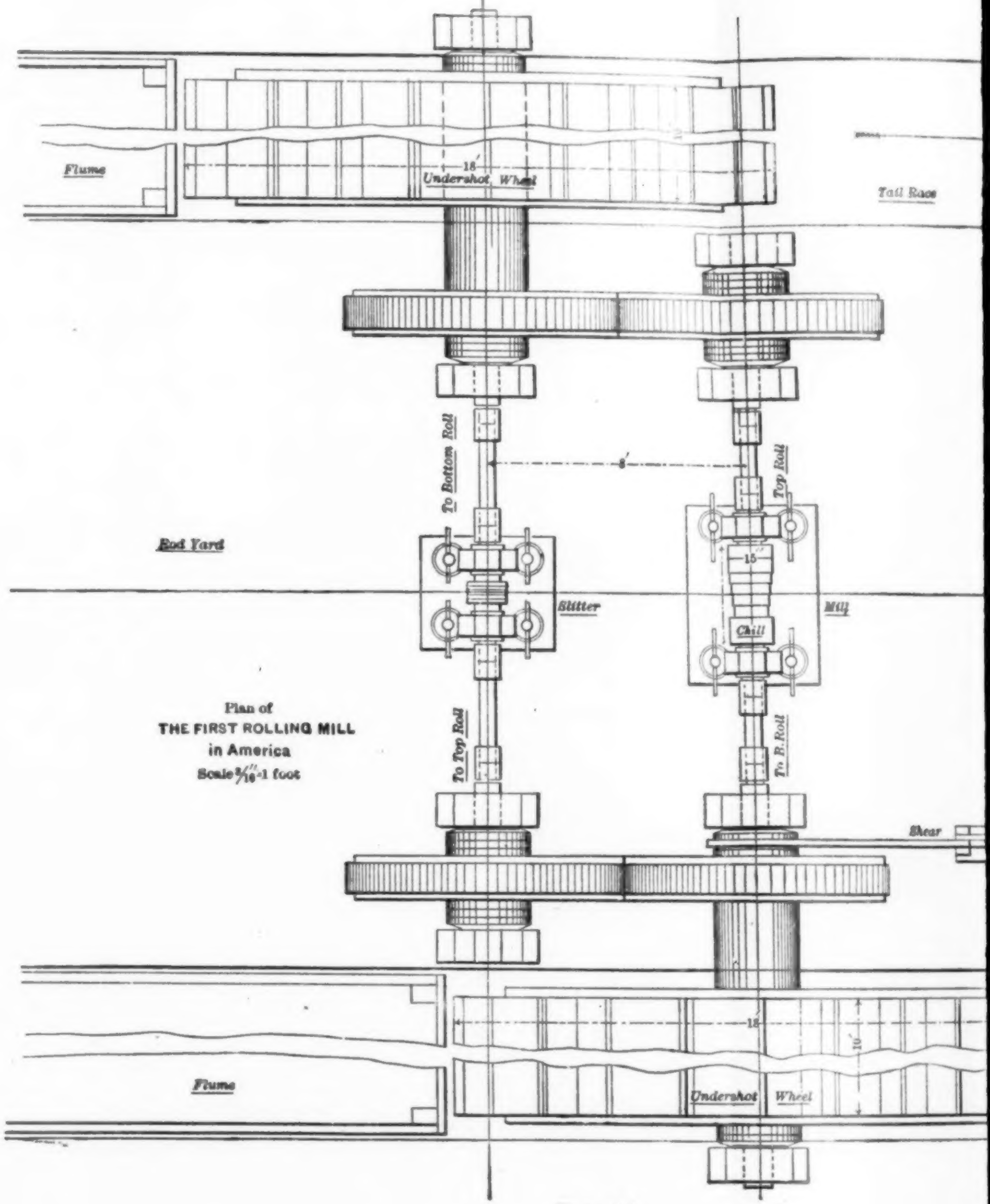


Fig. 26.

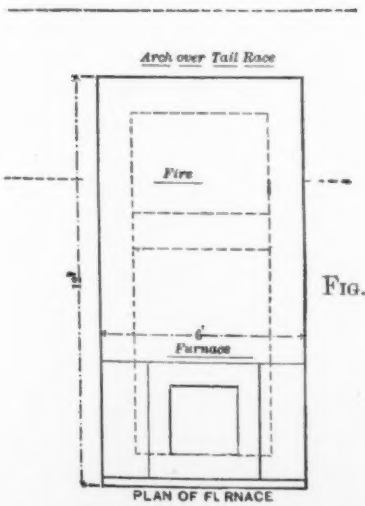
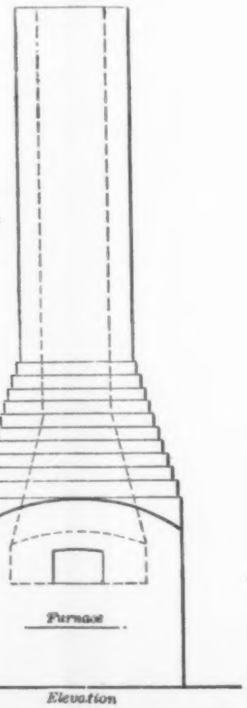


Fig. 28.

Fig. 29.



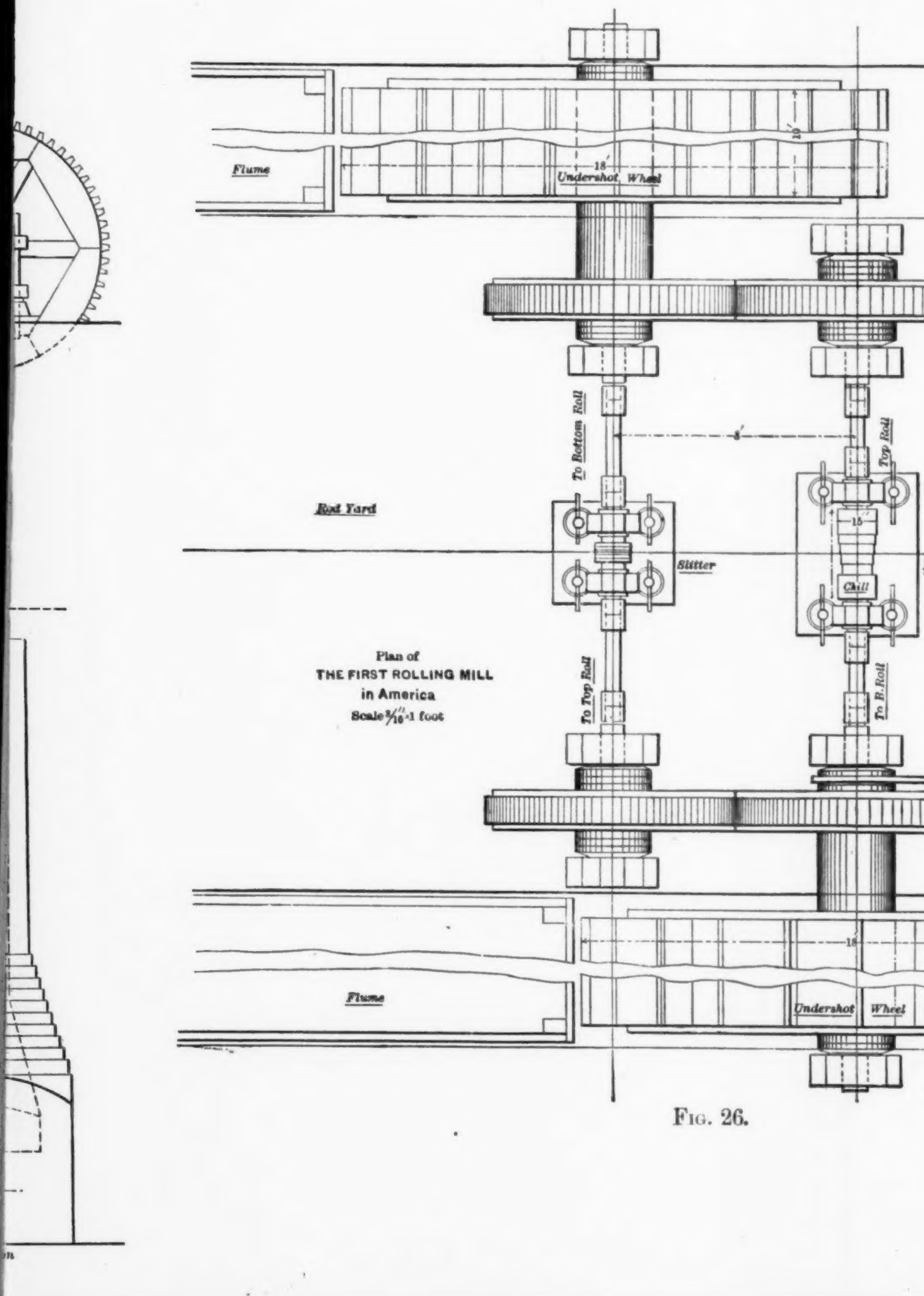
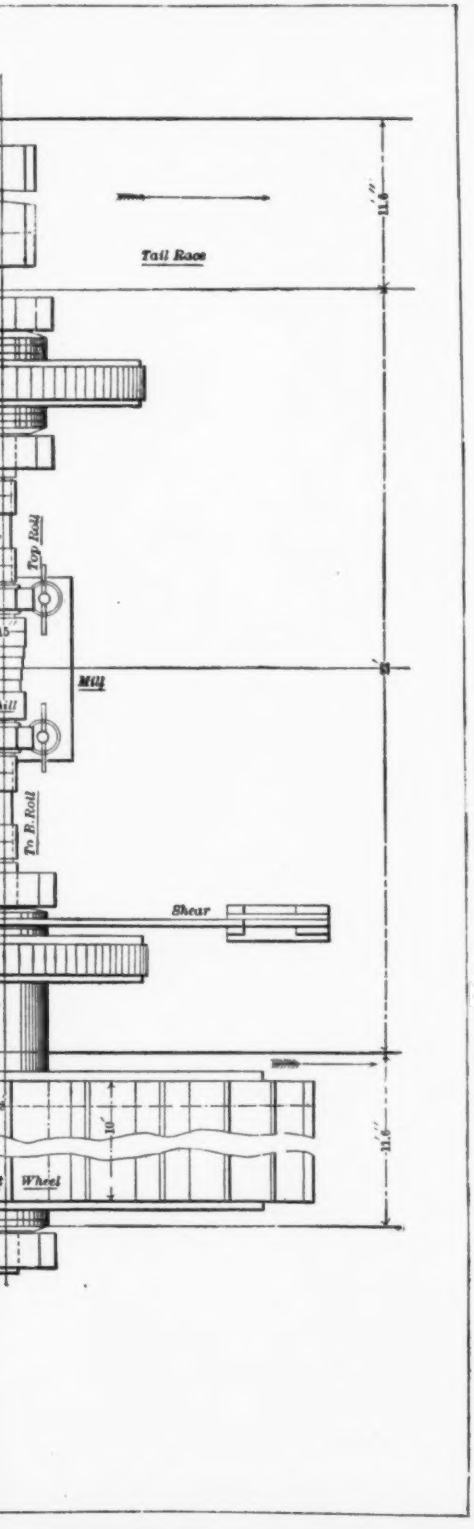

Flume
18'
Undershot Wheel
10'
To Bottom Roll
8'
Top Roll
Rod Yard
Slitter
15"
Chill
Plan of
THE FIRST ROLLING MILL
in America
Scale 3/16"=1 foot
To Top Roll
To B. Roll
Flume
18'
Undershot Wheel

Fig. 26.


11' 6"
Tail Race
Top Roll
Mill
To B. Roll
Shear
10'
11' 6"
Wheel

We have no reliable information as to who the man was, who did the actual planning, or the mechanical execution of this mill. The great man, who made the money, was Judge Peter Oliver, and his name has been handed down to us in the local histories, more on account of his political connections, than his knowledge of iron-works.

It is probable, from the fact of his being a native of Birmingham in Old England, that he had some knowledge of the processes carried on in that locality. At the time this mill was built, there was a law, prohibiting the making, importing, or using of machinery for rolling or splitting iron in the colonies, in order that the home manufacturers could control the American market. Subsequent events have somewhat modified the force of this prohibition! But Judge Oliver, in consequence of his political connections, was granted a special privilege to import and use this machinery. Hence it is supposed that this is the first rolling-mill erected on this continent. In the spring of 1776 Judge Oliver, along with a good many other of the loyal residents of Boston, went on board a British war-vessel, and sailed for England. The mill, by confiscation and sale, passed into other hands, and was kept in operation with varying success until 1830, when it was abandoned. The business of the mill was to roll down the hammered bars made at the charcoal forges—one of which was Leonard's Forge, built in the year 1651—into bars about $3'' \times \frac{1}{4}''$, and to slit them into nail rods of about $\frac{5}{16}''$ width, there being no such things as cut nails previous to 1818. There was then the rolling-mill and slitter, as shown. The bottom roll of the rolling-mill was driven by an undershot water-wheel, 18 feet diameter with 10 feet face, at the left-hand side, and the bottom roll of the slitter by a similar wheel at the right-hand side, eight feet further up the stream. The top roll of the mill was driven by a counter-shaft and 8-foot cog-wheel, gearing into a similar cog-wheel on the right-hand water-wheel shaft, and the top roll of the slitter by a similar gearing to the left-hand water-wheel shaft. The speed could be equalized between top and bottom rolls by raising or lowering the gates a trifle, until the bars would come through without turning up or down. The shear was operated by a wrought-iron lever, which was lifted up by a cam upon the water-wheel shaft, as shown in plan. The roll-stands consisted of bed-plates, as shown, each with four wrought-iron posts, 5″ diameter, keyed below; the bolsters, forming the top and bottom supports for the necks or journals of

the rolls, were all cast from the same pattern, with a hole at each end, by which they could be slipped over the columns and cobbled up with blocks and wedges, in the manner familiar to rolling-mill men. The rolls were tightened or loosened by driving the long keys at the top of the columns. The rolls were 36″ long, 15″ diameter at the ends, which were chilled. The necks were 9″ diameter. The iron was reduced in four passes from ¾″ × 3″ to ¼″ × 3″. The spindles were of wrought iron, 5½″ square. The water-wheels, cog-wheels, and shafts were of wood, with cast-iron gudgeons running in timber-boxes. The head of water in the flumes was about ten feet, and the speed of the wheels about fifteen revolutions per minute. The iron was received from the forges in bars, 3″ × ¾″ × 8′—0″. These were sheared into three lengths, heated in the furnace with a fire of pine sticks, and then rolled and slitted into the nail-rods. In the year 1818 Captain Zenas Crooker was the manager. About eight men were employed, at about $1 per day; six heats, of about eight hundred pounds each, were made in twelve hours' running. One pint of rum was consumed for each heat, or more, according to the weather. The value of the forge-iron was $100 per ton; nail rods, $120; and nails, $0.12½ cents, or nine pence per pound. The nail rods were put up in bundles of fifty-six pounds, and the nailers, who had their little shops around in the country, were expected to bring back fifty pounds of headed and pointed nails, receiving "store pay" of calico, tea, rum, molasses, etc.

About the year 1818, Jesse Reed, of Kingston, Massachusetts, brought out the machine for cutting nails, in pretty much the same form as it exists to-day. This did away with the old business of slitting, except for horseshoe nail rods; but the old mill was kept running, making flat plates, from which the nails were cut across the bar, the forge-iron being of such good quality as to admit of this treatment. Improvements in rolling followed, the timber fuel got scarce, and the old mill was abandoned and wrecked, so that to-day there is nothing left but the recollections of a survivor, which I have endeavored to put into tangible shape. The drawing was made under the supervision of S. Wilder, Esq., of New Castle, Pennsylvania, a retired iron manufacturer, who worked in this mill in 1818, and gave the writer the principal dimensions in feet and inches, and the method of operating. As to whether this mill, in the year 1818, was precisely the one built in 1751, Mr. Wilder states that it is likely that there had been some re-

newals of the wood-work, but most of the iron-work was the original. It was impossible to break down the mill, from the fact that, if a heavy piece or a pair of tongs were passed in, the effect would be, after some squeaking of the timber-wheels, to stop everything.

DISCUSSION.

MR. HOLLEY: I would like to ask Mr. Harrison if he knows whether these rolls were simply plain rolls?

MR. HARRISON: Plain offsets.

MR. HOLLEY: No box grooves?

MR. HARRISON: None; I inquired particularly about that; it seemed a little odd. The passes were about four inches wide, and the guides were so placed—the bar being about three inches—that they gave plenty of bearing without turning a fin over the edge. After the cut nails came in, these chills were used for rolling down the plates.

MR. HOLLEY: I thought you said that the plate, after rolling, was of the same width but different thickness.

MR. HARRISON: Those plates rolled down from about three-quarters thick to about one-quarter.

MR. HOLLEY: Was there any edging?

MR. HARRISON: There was no edging at all, but the iron was passed from here (*indicating*). The shafts were eight feet apart. The roller was here, and the catcher was here (*indicating*). After coming through the last pass the attendant would send it off through the slitter; and here was the place called the rod yard, where they made up the fifty-six pound bundles.

XXX.

THE SUPERIOR.

BY E. D. LEAVITT, JR., CAMBRIDGEPORT, MASS.

THE subject of the following paper is, The Compound Hoisting Engine, now building by the I. P. Morris Company, of Philadelphia, for the Calumet & Hecla Mine.

It is the fourth engine of its kind ordered by the Calumet & Hecla Mining Company, for whom the design was first specially

made in 1874, and by reason of greatly exceeding its predecessors in size and power, it has been named the "Superior."

A brief history and description of this type of engine may be of interest.

Engine No. 1 was built in 1874 for pumping the water required by the separating and dressing machinery at the Company's stamp mills, which are situated at Lake Linden, Michigan.

It was an outcome and departure from the Lynn pumping-engine; the arrangement of cylinders with their outer ends placed near together, and their pistons connected to opposite ends of a beam, being retained, together with the valve gear.

The departure from the Lynn engine consisted in inverting the cylinders, placing the beam below them, with its journals carried in pedestals on the main bed-plate, and so constructing the beam that the connection from it to the crank was situated above and between the piston connections. The position of the shaft was by this means brought very nearly in the same horizontal plane as the main beam centre, its distance from the same being a little greater than the length of the connecting-rod.

Two pumps were used in this engine, one being located at each end of the beam, and both were hung to, and below, the bed-plate. The latter consisted of a pair of deep hollow girders, placed parallel with each other and bolted at their ends to heavy foundation piers. This arrangement afforded excellent facilities for getting at the pumps.

It should be stated that Engine No. 1 had its low-pressure cylinder placed vertically, and its high-pressure cylinder at an angle of about 15° from the vertical. One pump was situated in the same centre-line with the low-pressure cylinder, thus allowing the steam and water pistons to be rigidly connected. The pump at the high-pressure end of the beam was also vertical and worked by a trunk connection from the beam.

Although the first engine was built with the intention and expectation that it would have a large surplus capacity, the additions to the works were so large before it was put in operation that it had to start at a speed greater by twenty-five per cent. than its intended maximum, and before it had run one year this speed was nearly doubled.

The great demand for water at the stamp mills, and the success of the first engine in supplying it, decided the authorities of the Calumet & Hecla Company to build a second pumping-engine, in

1876, of more than twice the nominal capacity of the first, and a third engine, to be used for hoisting and general machinery-driving at the Hecla Mine.

The second engine was very like its predecessor, save in the construction of the pumps. The latter were fitted with differential plungers in lieu of buckets and plungers, and the change has proved a good one.

Pumping-engine No. 2 was put in regular service early in December, 1876, and has run constantly one hundred and forty-four hours per week since that date, save on weeks having legal holidays, and one entire week when it was laid off to test the pumps of Engine No. 1, after they had been fitted with improved valves.

During the period of its operation it has made more than forty million revolutions, and raised, approximately, thirteen thousand million gallons of water against a dynamic head of fifty-one feet.

It has neither required nor received any repairs.

The dimensions of the cylinders of Pumping-engine No. 2 are $1'$–$5\frac{1}{2}''$ and $3'$–$0''$ diameter by $5'$–$0''$ stroke, and of the pumps $1'$–$8''$ and $2'$–$4\frac{5}{16}''$ diameter of plunger by $5'$–$0''$ stroke. The usual speed is twenty-four revolutions per minute.

The third engine was erected at the Hecla Mine in the fall and winter of 1876 and 1877, and started early in 1877. It is employed to drive four hoisting drums, each twenty-four feet diameter, and weighing seventy tons; also a pair of $28'' \times 48''$ air compressors, the rock-breakers and other mine-machinery.

Its load is exceedingly variable, running from flying light to six hundred and fifty horse-power. The cylinders were originally $1'$–$6''$ and $3'$–$0''$ diameter by $5'$–$0''$ stroke; but, for the purpose of increasing the power of the engine, two new cylinders have recently been made, having diameters of $1'$–$10\frac{3}{4}''$ and $3'$–$2''$.

The Hecla Hoisting Engine makes forty-eight revolutions per minute, and runs twenty hours per diem.

The extreme variability of the load was considered a condition directly opposed to a first-class economy, and the constructor's guarantee was four pounds of Brier Hill coal per horse-power per hour, the coal to be of such quality as should evaporate 7.74 pounds of water per pound of coal from and at 212°. On a trial of five days in May, 1877, the coal averaged 3.125 pounds per horse-power per hour, and the evaporation 7.42 pounds of water per pound of coal from and at 212°.

During a trial extending from December 21st, 1880, to January 1st, 1881, the consumption of coal was found to be 2.13 pounds, and of feed water, 16.3 pounds per horse-power per hour. The power during the latter averaged more than double the average exhibited on the preceding trial.

In 1878 a design was commenced for an engine of 1500 H. P., to be located at the Calumet Mine. Before the drawings were completed it was deemed judicious to increase the size of the engine to 2500 H. P., and a contract for the same was entered into in the fall of 1879.

In designing this engine, the economical advantages of "high-pressure" and high-piston speed were carefully taken into consideration; 135 pounds per square inch was adopted for the former, and 720 feet per minute for the latter.

As the load when the engine is first put in service will be, on the average, but about one-fourth of its rated power, extra precautions have been taken to provide against losses due to internal radiation. To accomplish this, the cylinders have been thoroughly steam-jacketed on sides and ends, and the exhaust from the high-pressure cylinder is passed through chambers filled with small brass tubes, through which steam of boiler pressure circulates.

There are two of these chambers, which are called re-heaters, placed between the cylinders, one at either end, and each contains about 700 square feet of heating-surface. The arrangement is shown in the side elevation of the engine. See Figs. 30 and 33.

The "Superior" has both high-pressure and low-pressure cylinders placed vertically, the distance between centres being 9′-0″. The diameter of the high-pressure is 3′-4″, and of the low-pressure 5′-10″. Each has 6′-0″ stroke of piston, and the speed will be sixty revolutions per minute.

These heaters that connect the cylinders are oblong chambers in their transverse and vertical dimensions, as seen in Figs. 34 and 35. Each contains 941 brass tubes, $\frac{5}{8}$″ diameter, and 60″ long. The live steam is inside, and the exhaust outside these tubes. By the use of diaphragms all the tube-surface is made efficient.

The distribution of steam is effected by gridiron slide valves, as seen in Fig. 36, which have a short horizontal travel. There are four valves for each cylinder, each being actuated by a separate grooved cam, so that its movements can be independently controlled and adjusted.

The valve-gear consists of eight grooved cams, fixed on a re-

volving shaft, which is driven by mitre gears from the main crank-shaft. See Figs. 30, 31, and 32.

The cam-shaft is carried in bracket bearings, which are secured to the side of the main bed-plate, and in addition have feet which rest upon the engine foundation.

The cams give motion to levers, which in turn communicate it to the bell-cranks that move the valves. The connections between the levers and bell-cranks are tubular.

The cam-levers are made of Chester cast-steel; their pins and rollers are of hardened open-hearth steel. The throws of the cams are also of hardened steel, as shown on the drawing.

The cut-off is effected by the high-pressure inlet-valves, and is automatically controlled by the governor. The range is from 0 to 0.6 the stroke. This is accomplished by making the cam in two parts, one for opening and the other for closing the valves. The opening part is made fast to the cam-shaft, while the closing part is driven through a sleeve, which has a key-slot running its entire length. The shaft inside the sleeve has a spiral key-way, and a key is fitted to it and to the slot. This key is also made fast to a sliding collar, which embraces the sleeve, and is capable of being moved back and forth by a yoke and lever. The lever receives its motion from the piston of a small steam-cylinder, the valve of which is operated by the governor in such a manner that the position of the piston is definitely fixed by the position of the governor-balls. Abundant power for moving the collar is thus provided, in combination with a very delicate and sensitive Porter governor to determine where and how far it shall be used.

The cam just described operates two independent compound levers, one being required for each high-pressure inlet-valve. The compound levers are so constructed that one part has a fixed pivot at one end and a roll fitting the opening cam at the other. At the middle of its length there is a stud, to which the other part of the lever is pivoted. This lever is provided with a roll that fits the movable or closing cam, which is situated about one-third of its total length from the pivoted end. The outer ends of these second levers are connected to vibrating cams that move the high-pressure inlet-valves, the function of the vibrating cams being to allow the large amount of lost motion that is indispensable in obtaining a quick movement. As the travel of the inlet-valves is but one inch, while that of the vibrating cams is twelve inches, a very sharp cut-off results.

The low-pressure inlet-valves are set to close at thirty-five inches from the commencement of the stroke. A full-sized drawing of the cams that move these valves (not here shown) answers for an example. The other cams are similarly constructed. It will be observed that the disc part of the cam is attached to a centre that is keyed to the cam-shaft. These centres have a T-shaped groove, into which the bolt-heads that secure the discs are fitted. This construction allows any amount of adjustment that can be desired, as will readily be seen.

The hand-gear consists of a pair of small steam cylinders, which turn a crank-shaft that can be connected at will with the cam-shaft by a clutch, the axes of the two shafts being in the same line. The mitre-gear on the cam-shaft also has a clutch, which is connected to the clutch on the small crank-shaft in such a manner that it must be thrown out of gear before the other can be put in gear, and *vice versa*.

The eccentrics for working the valves of the small cylinders are placed on a separate shaft, which is turned by a hand-wheel. The eccentrics give motion to levers which are pivoted to other levers that receive motion from the pistons. A differential motion is thus imparted to the valves, with the result that the engine will stop immediately when the hand-wheel ceases to turn, because the motion imparted by the levers connected with the pistons will close the valves.

The main framing of the engine is formed by four massive columns, which are secured to the bed-plate, and have suitable flanged facings at their upper ends, to which the feet of the cylinders are bolted. These columns also form the cross-head guides.

The bed-plate is in four pieces, two sides and two ends, the back end forming the air-pump and channel-way.

The jaws for the crank-shaft and beam pedestal-boxes are cast in side pieces of the bed-plate, all the journals being situated in the same horizontal plane. The gross weight of the bed-plate is sixty-five tons, each side piece weighing twenty-one tons.

The running-gear of the engine, with the exception of the beam and crank-throws, is of steel. The piston-rods are respectively $6\frac{3}{4}''$ and $7''$ diameter. The cross-heads, low-pressure link, connecting-rod, main-centre, crank-pin and shafts, were made by Krupp, of Essen, and are beautiful work. The low-pressure link and the connecting-rod are of a peculiar construction, as will be seen in Fig. 30. A jaw is formed for the boxes by

cutting in from the side. A binder is then fitted, so as to hold the jaw from opening or closing, and the adjustment of the boxes is effected by a wedge, which is drawn up by a screw and nut; two set screws bearing against the upper edge of the wedge hold it solid. The boxes are also held solid by set screws in their flanges.

The beam—or perhaps the term rocker would be more appropriate, see Fig. 30—consists of a pair of gun-iron wheels, 11′-0″ in diameter, with the pins for piston and crank-connections forced into their rims. The main centre is 1′-6″ diameter on the body, and 1′-3″ diameter in the journals, which are 2′-6″ long.

A heavy counterbalance casting is bolted to the bottom of the beam-wheels, its weight being adjusted so as to equilibrate all the vibrating parts. This is a matter of importance, as these weights aggregate upwards of twenty-five tons, and make a trifle over two vibrations per second.

The crank-shaft is 1′-6″ diameter in the journals, and 45′-0″ long, over all, the journals having a length of 2′-8″.

The crank-pin journal also is 1′-6″ diameter, and has a length of 2′-0″.

The crank-throws are made of the best charcoal scrap-iron, and weigh four tons each.

The total weight of the shaft is nearly thirty tons.

The air-pump is horizontal and double-acting, 2′-6″ diameter, and 2′-6″ stroke. It is worked by an arm on the end of the main-beam centre. It is of the plunger construction, and has water-packing. The valves are of rubber, and have iron grids for seats.

There are two pulley fly-wheels, each 32′-0″ in diameter, 2′-8″ face, and weighing forty-five tons. Each consists of a centre and twelve segments, an arm and a segment being cast together. As a precaution to resist danger from centrifugal force, the rim has twenty-four wrought-iron braces, 3′ × 1″, in section for securing it to the centre.

Each pulley will carry a thirty-inch treble belt, which will have to transmit a maximum of 600 H. P., to be distributed by wire rope transmissions over a distance of two thousand feet.

One end of the crank-shaft will be coupled to the pinion-shaft that drives the hoisting-drums, and the other end to a pinion-shaft for driving two pairs of 32 × 48″ air-compressors.

All the shafting is made of steel, and varies from 1′-0″ to 1′-6″ diameter, being proportioned to transmit with entire safety the maximum power of the engine, which is estimated at 4700 H. P., when working with six expansions. It is hardly necessary to remark that such an amount of power is not likely to be called for very shortly.

The foundations are constructed of brick and stone masonry. They are forty feet long, eighteen feet wide, and eighteen feet deep above the pocket-holes (for bolts). They rest upon rock bottom.

For supplying steam to the "Superior," there will be five locomotive boilers, containing in the aggregate 260 square feet of grate, 11,000 square feet of fire surface.

The dimensions of the boilers are as follows:

Length, extreme, ...	$33'-4\frac{13}{16}''$
Breadth of fire-box at bottom, ...	$9'-2\frac{1}{8}''$
Height of fire-box at extreme, ...	$8'-\frac{9}{16}''$
Inside diameter of barrel, ...	$7'-0''$

Each boiler has two furnaces 8′-0″ long, 4′-0″ wide, and $5'-6\frac{1}{4}''$ high, above base ring. Back of each furnace is a combustion chamber, 2′-11″ long extending into the barrel of the boiler, where the two unite in a single chamber, 4′-1″ long, up to the front tube sheet.

There are 118 iron tubes, $3\frac{1}{2}''$ diameter outside, and 18′-0″ long between tube sheets.

All the plating is of the very best quality of open-hearth steel, varying from $\frac{5}{16}''$ in thickness, as used in the furnaces, to $\frac{9}{16}''$ in thickness, for the circular shell.

All the joints of the barrel are butts, with outside and inside straps; and are double and treble riveted throughout.

The staying is proportioned for a working pressure of 135 pounds per square inch. The crown-sheets are hung by bolts to wrought-iron arches, which are riveted to the roof of the boiler; and the bolts are so arranged that a man can pass the entire length of the crowns and examine every part. Access can also be had to the barrel, below the tubes, for its entire length.

The boilers are supported by a cast-iron ash-pit, at the furnace-end, and three cast-iron cradles under the barrel. The cradles rest on iron balls, so as to move freely to accommodate the expansion.

There is no brick-setting. To prevent radiation there is careful clothing with non-conductors.

Each pair of boilers will have a flue-heater for the feed-water. These heaters are composed of brass tubes, $2\frac{1}{2}''$ inside diameter, having several hundred square feet of exposed surface. By this method, the water enters the boilers at 140° and upwards.

The main steam-pipe is of wrought-iron tubing, $1'-0''$ diameter, and about $75'-0''$ long. It discharges into a receiver, near the engine, which is $5'-0''$ in diameter, and $15'-0''$ high. A short pipe, $1'-0''$ diameter, connects the receiver with the high-pressure cylinder.

It is expected that the "Superior," with three of its boilers, will be completed the present season.

In conclusion the writer desires to put on record the fact that the creation of the plant to which the "Superior" forms the latest and greatest addition, is due to Professor Alexander Agassiz, the President of the Calumet & Hecla Mine. An engineer by profession, he has the great advantage of being able to readily grasp engineering details, and thus fully comprehends what is often lost sight of: the axiom, that the best is the cheapest.

DISCUSSION.

Mr. Leavitt said that he would mention *en passant*, for the benefit of American artisans, that Krupp finished the shafts, which were of a beautiful bluish tint, without perceptible blemish, but they were found to be quite out of round, and had to be refitted.

Mr. Hill: Mr. Chairman, I would like to ask, what to me is one of the most important questions, relating to the intermediate cylinder that Mr. Leavitt has placed between those engines, what is the reason for using brass tubes instead of iron? Also, for what reasons he determined upon the size of heating surface? This question of superheating is, to my mind, one of the most important questions we are likely to deal with in the future, and all the points that will help us in that direction seem to me most valuable.

Mr. Leavitt: The reason for using brass tubes is that they can be obtained one thirty-second of an inch thick, and are not as likely to fur up in use; as regards the quantity of heating surface, we put all we could in the given dimensions; I did not wish to have the reheater space too large. There are some reheaters used on

the Boston sewage engines, and the surface, per horse-power, would probably be about five times as great in that case as here. We had to do what I regarded as rather objectionable—viz., twist the steam about in getting it to the low-pressure cylinder. I have no doubt that if more surface could have been applied, per horse-power, it would have been better.

Mr. Stirling: I am glad that attention has been called to the advantage of a receiver between the boiler and the engine. I think that we lose a great deal of our power by not maintaining the boiler pressure on the valves of the engine. I think it is very important, where steam is carried any distance at all, to have receivers near the engine.

I failed to notice, as the paper was read, the maximum amount of horse-power. I would like to ask how much that was; and, in relation to the amount of heating surface given in the boiler, I notice that the "Gallia" of the Cunard line has 13,000 feet of heating surface and indicates over 5,000 horse-power, which is an allowance of 2.5 square feet of heating surface per horse-power. And I would like to see how this engine compares with that. That seemed to me to be remarkably low. I would like also to ask Mr. Leavitt—incidentally he made the remark that the valve shafts had to be very rigidly supported on the foundation—I would like to ask him the reason of that, and if he has made any arrangement for balancing the valves of this engine.

Mr. Leavitt: In regard to the proportion of heating surface, it was made large in order to secure a high evaporation. The maximum power—cutting-off at half-stroke in the high-pressure cylinder, with 135 pounds boiler pressure—is estimated at 4700 horse-power. There is a possibility that we may, in the future, require that amount at short periods; when, for instance, we get all our skips at the bottom of the mine together; I made a calculation with regard to our 70-ton drums a little while ago and found that it required 1300 horse-power for one revolution, to put them in operation; we now slack up of necessity, and the fly-wheel distributes this initial power over several revolutions, but as the mine grows deeper we cannot afford anything of that kind.

The engine must then keep up to speed all the time or we cannot hoist the material required.

This matter of rigidly supporting the valve shaft is on account of the large diameter of the cams, which will not admit of any deflection in the shaft.

In regard to balancing the valves, I have never given that matter much thought. I know that these valves work well enough for all practical purposes. They take very little power. The movements are short and instantaneous, and from the size of the valve gear that we have been compelled to use thus far I think they have worked very easily. There is machinery enough about the valve-gear any way. We do not want any more.

Mr. Emery: I must say it is very gratifying to me as an engineer to look over plans of this kind and see the thorough manner in which the work has been carried out. There seems to have been plenty of means to do a good job and the proper head to do it; and those two elements brought together are sure to make a success of a matter.

In regard to this superheating between the two cylinders it has been agitated for years, as is well known; and some seven years ago I made a design for an engine of large size for a large manufacturing establishment—Mr. Weightman, here present, making the drawings for me. It was a little different in design, but carried out many of the same features shown in Mr. Leavitt's plans, including the superheating between the cylinders. My engine was never constructed, for a reason that some here can appreciate. It is never well, in dealing with certain classes of people, to press matters—wait for them to come to you, rather than drive them up in drummer fashion. That was at least the view that I took. I considered that the parties had confidence in me, but it turned out in the end, somebody else wanted the work and drummed for it, and finally put into the establishment the oldest and simplest form of engine without superheating, compounding, or furnishing even a governor cut-off; and that is running there to-day. It is something of a disappointment, as the parties had the means, and if properly guided would no doubt have done something well. While the views of different engineers run in different lines as to the way the details should be put in, still the principles which underlie the matters—the method of constructing engines, getting small clearances, obtaining the superheating, and all that—would necessarily be much alike. My views at that time, which I should not change now, were in the direction of connecting the two cylinders to the same crank-shaft at right angles, with a comparatively large receiver between. In that case there is no necessity of reducing the capacity between the cylinders, as there is to a certain extent with the kind of engine where you connect them opposite; because by

cutting off at a proper point in the second cylinder,—the larger of the two,—there will be compression sufficient in the small cylinder to bring the pressure back to the point to which it is reduced by expansion in the second cylinder. Whatever has been done in that line elsewhere seems to show that this form is as correct at least as any other, and I think, and in fact it would almost appear to a mechanic without much discussion, that the details necessary to carry out the work are somewhat simpler and cheaper. I do not suppose in the matter of efficiency there is any great difference.

Mr. Henning: I would like to ask Mr. Leavitt about the bell-crank he mentioned as having been made of cast steel; whether such castings can be depended upon, and are free from blow-holes, and defects of that sort?

Mr. Leavitt: I think we had five or six sets made before getting them free from blow-holes, and things of that sort, but we finally succeeded in getting them sound. We started with the idea of having that work all of steel. Cast iron probably would have been strong enough by making it a little heavier. The principal reason, though, for making it of steel was that we wanted to get a long connection between the eyes of the levers, and we could make of steel a great deal thinner flange than we could with the cast iron, and could thus get a shorter width of bell-crank.

Mr. Holley: The fact of our not having machinery in this country to forge such large shafts as those which have been mentioned, might be deemed discreditable to our steel-makers. But there has not been demand enough for heavy forgings to warrant the erection of big hammers. But the important point is that steel castings are now so perfected as to be more trustworthy than forgings. It is a fact that some rolling mills, on the other side of the water, prefer castings to forgings for their reversing engine shafts. The Terrenoire works were the first who took up this branch of manufacture in a scientific manner. By the use of silicon and carbon the steel could be made sound. Unfortunately, when castings were sound they were not strong, and when soft and tough they were not sound. The silicon, which by decomposing carbonic oxide prevented the formation of blow-holes, remained in the metal as silica and reduced the strength. The next step was to form a fluid slag by which the silica could be removed. This was accomplished by the introduction of manganese, which, with silica, forms a very fluid slag. This washed the metal free from silica, and also from oxide of iron, which is a strength-reduc-

ing element. The results obtained by this process are very remarkable—the metal is made of both hard and soft grades. The hard metal has most extraordinary properties. Its average elastic limit was 60,500 pounds, while the breaking strength was 126,000 pounds. The softer metal showed an elastic limit of 30,200 pounds, with an ultimate strength of 90,000 pounds. The elongation was in these samples from two to three per cent. in the hard metal, and about thirteen per cent. in the soft. The hard metal, by reason of its high elastic limit, and in spite of its hardness, is adapted to a variety of purposes, like projectiles, and to many engineering structures. The soft metal, however, is really very strong, and being homogeneous requires a smaller factor of safety than would be safe in forgings. One of the advantages of this process is that there is no limit to the size of the mass that can be cast, for many furnaces can pour their charges into the same mould. On account of the great shrinkage, soft cores are necessary to prevent the metal from being pulled apart.

Mr. Henning : The question I asked about steel came up to me in the way of inspecting the steel for the East River bridge, because very eminent engineers in this country have put their opinions against the use of steel castings, and they have advised the engineers of that bridge not to use any for fear of having flaws in the material ; and I believe such opinions as I have heard expressed just now will lead to the adoption of cast steel in some detail of the bridge. If I could get some more opinions from eminent engineers on that subject, I am sure they would have great weight in that direction. In inspecting the steel I have found that in the Bessemer steel used in that bridge the elastic limit is from 42,000 to 47,000, neither above nor below ; the ultimate strength is generally 73,000, none below 72,000, and very little above 90,000, those being very hard specimens, and being hard from water dropping on the material while rolled. But if the material in the casting has an elastic limit of 30,000, it certainly appears that it would be suitable for almost any structural purpose. The part I speak of is a detail which is really more machinery than anything else, and it would be difficult to make it any other way except by casting. I would be very glad to know from any of the gentlemen whether they have had any experience in this matter of steel castings, unless it is out of place at the present moment.

Mr. Emery : I would ask in that connection what the elongation is of the steel you are using in the bridge ?

Mr. Henning: Not below fifteen per cent. nor above twenty-five generally.

Mr. Emery: An elastic limit of 40,000.

Mr. Henning: 45,000,—the elastic limit is more nearly 43,000, I would say, than anything else, with an elongation generally of nineteen per cent. In plates I should say of course that it is considerably lower; but in channels and angles and rods we obtained over fifteen per cent. elongation. In flat bars we have obtained very nearly twenty-five; and in all this work—now about 1500 tons have gone through the shops—we have found but a few single pieces which were hard or brittle so that they could not be used; but in almost every case the simple application of a punch proved that that material was unfit for use. It either broke the punch or it broke the material; and the surface of the material when sheared or punched showed the crack pretty closely. The material runs very uniformly, and it seems as though the objections to hard steel being introduced in any structure are exaggerated by almost all who have not used it. The steel is more uniform than any wrought iron that has been used in very large bridges; the tests show that in the first place before rolling, then after rolling, and lastly when the material has gone through the shops. The working of the steel has a great effect upon it. Of course no drifting must be allowed in any case, and, as far as the inspectors can see to it, it is prohibited, and I am proud to say that they are quite successful. The working of the steel in the hydraulic presses, or under heat, has given considerable trouble without annealing; and when the steel is annealed it goes back to its original condition very nearly. All other work on the material has proved it superior to anything else. It works better than almost any kind of iron. The heating of the steel without annealing seems to have a deteriorating effect, which has not yet been quite overcome; although we hope that before any amount of work of that kind has been done on it, experiments will be made sufficient in number and character to find a method by which that steel will be worked without any injurious effect whatever to it.

Mr. Emery: You mean local heating instead of heating the whole place.

Mr. Henning: Local heating I mean.

Mr. Oberlin Smith: I have used several hundred castings, weighing from one to five hundred pounds, chiefly for crank and cam-shafts, in a place where there is a violent jarring and in some

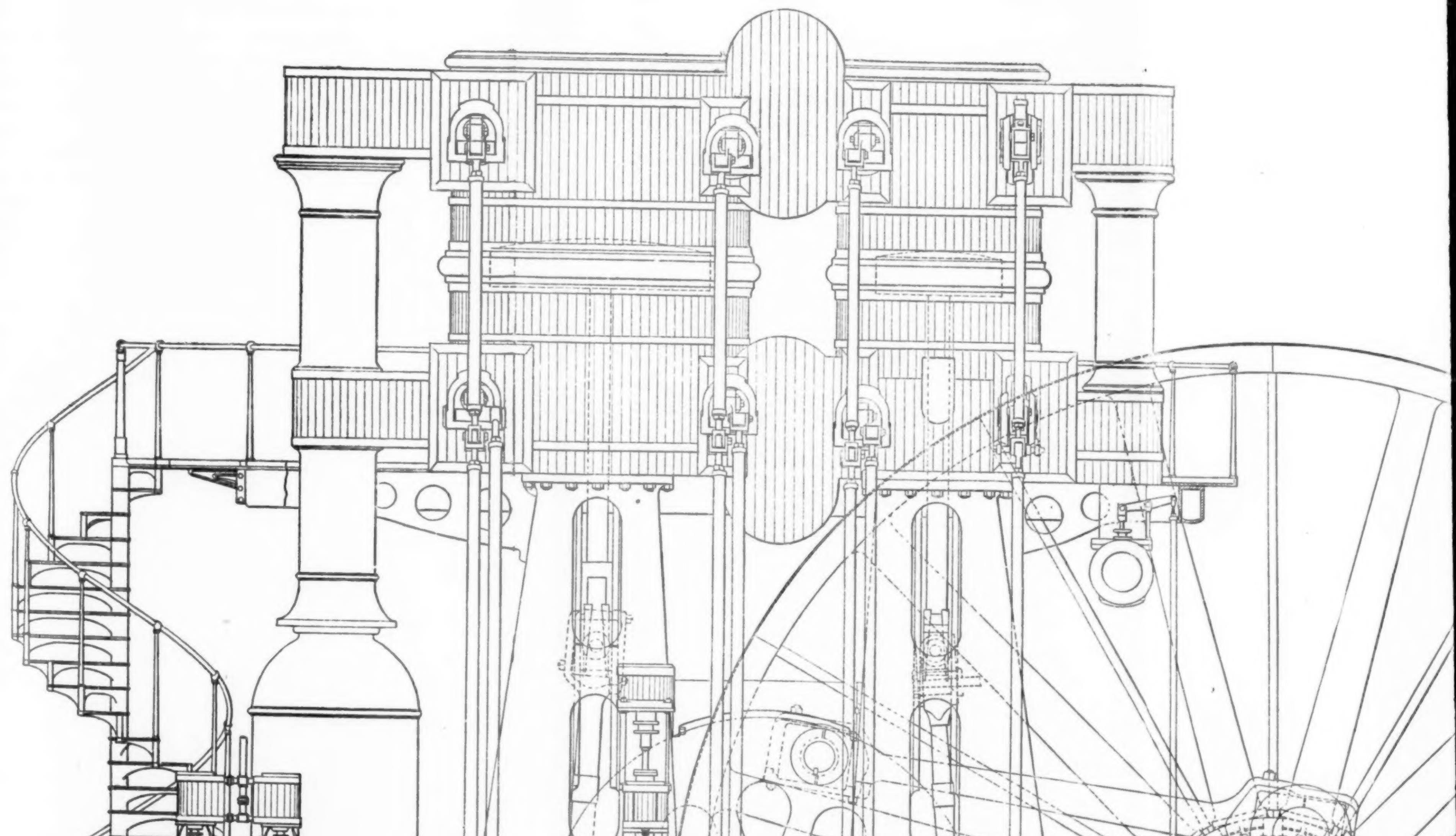
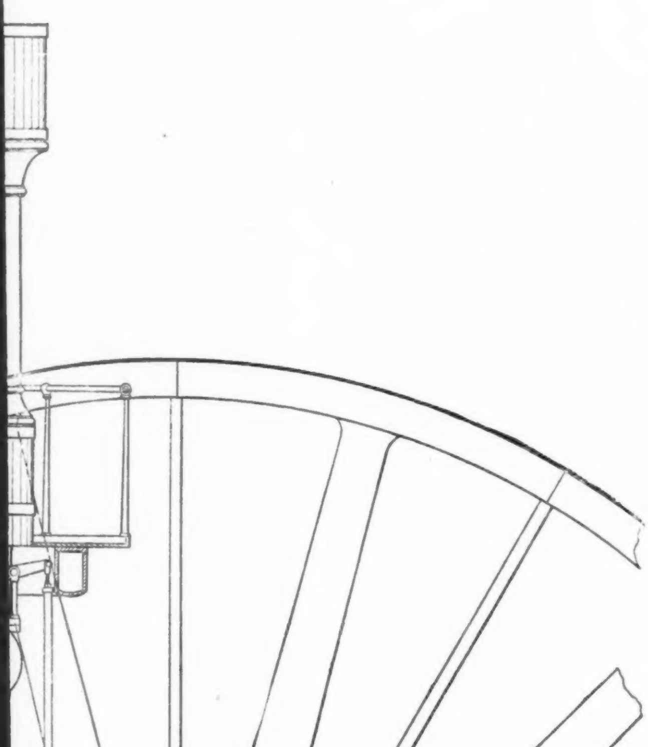


FIG. 30.—SIDE ELEVATION OF COMPOUND HOISTING ENGINE, SUPERIOR.



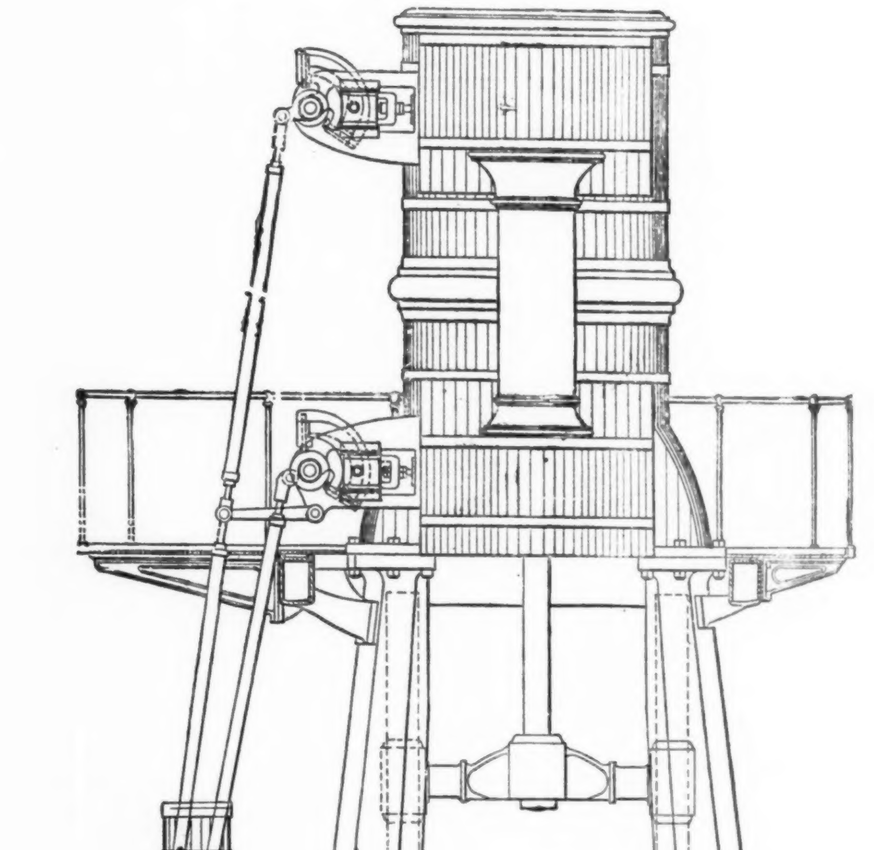


FIG. 31.—END ELEVATION OF HIGH PRESSURE ENGINE, SUPERIOR.

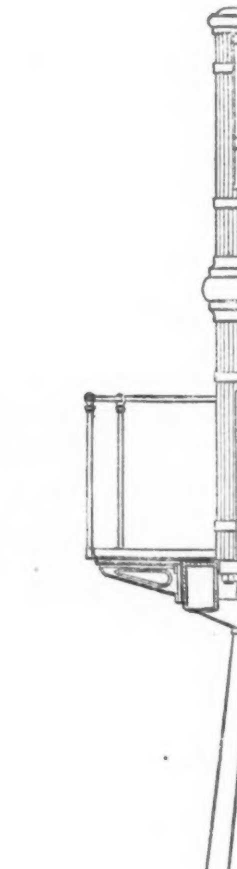
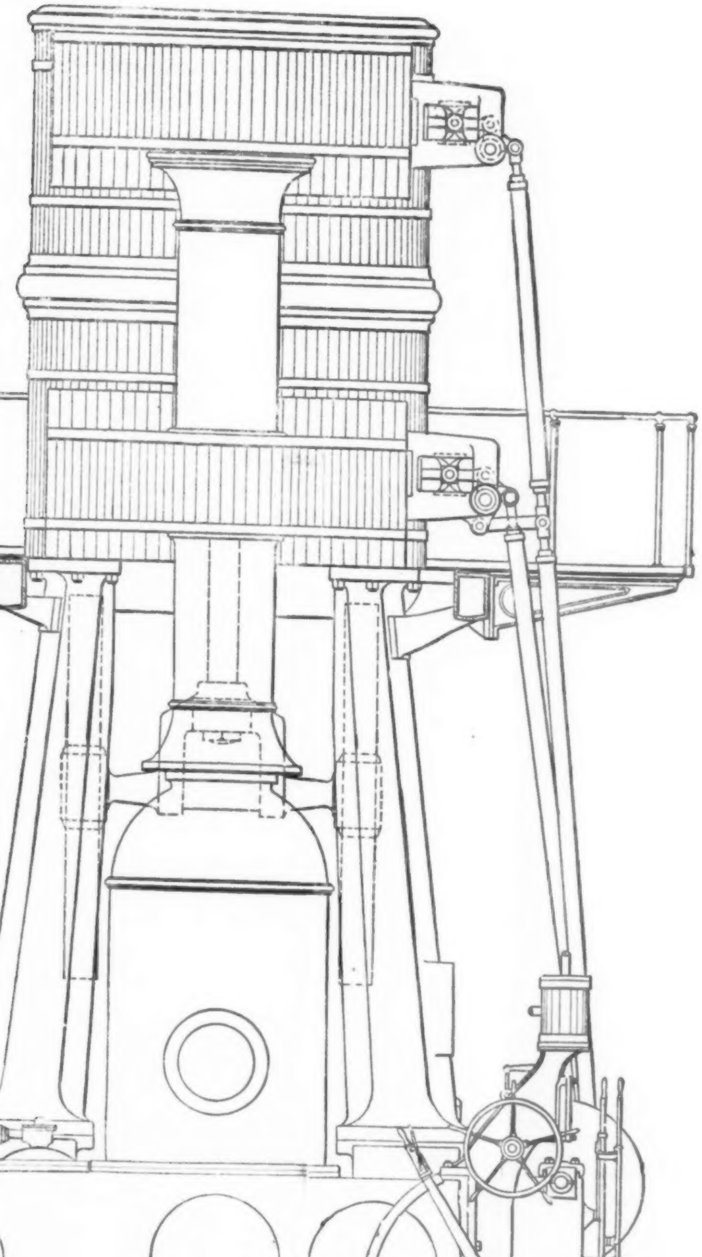


FIG. 32.—END E



ELEVATION OF LOW PRESSURE ENGINE, SUPERIOR.

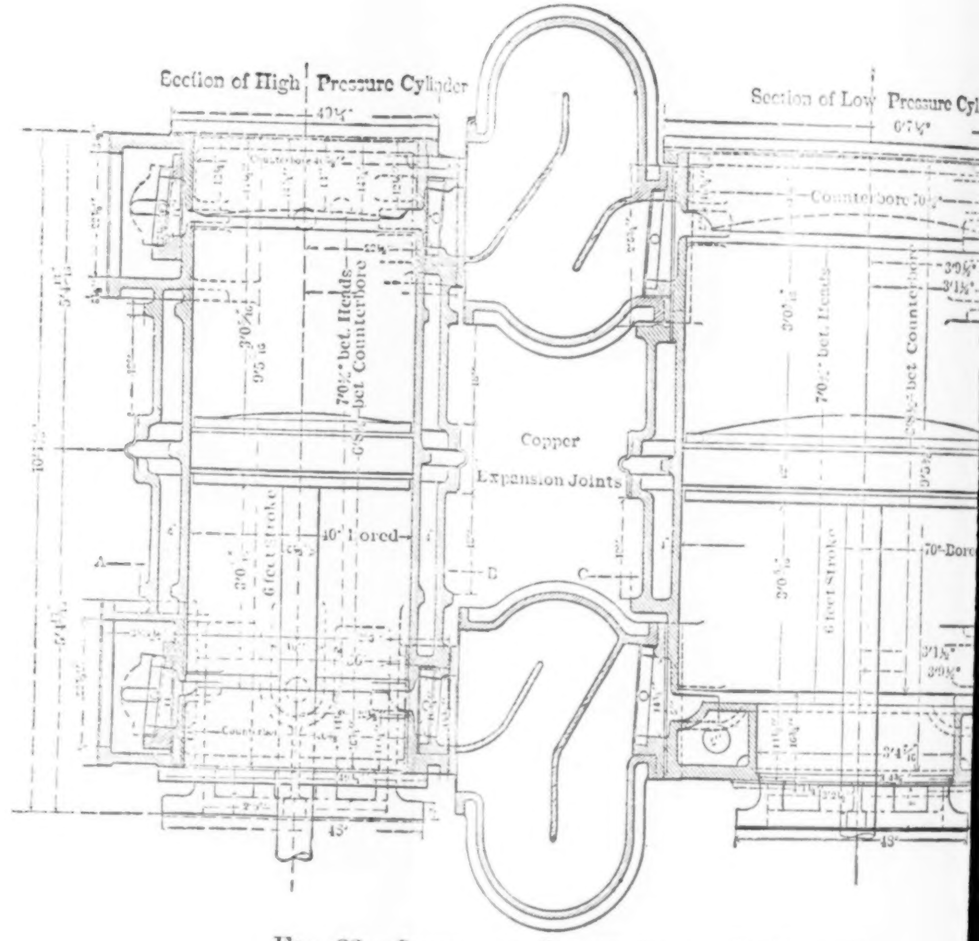


FIG. 33.—SECTIONS OF CYLINDERS AND RE-HEATERS.

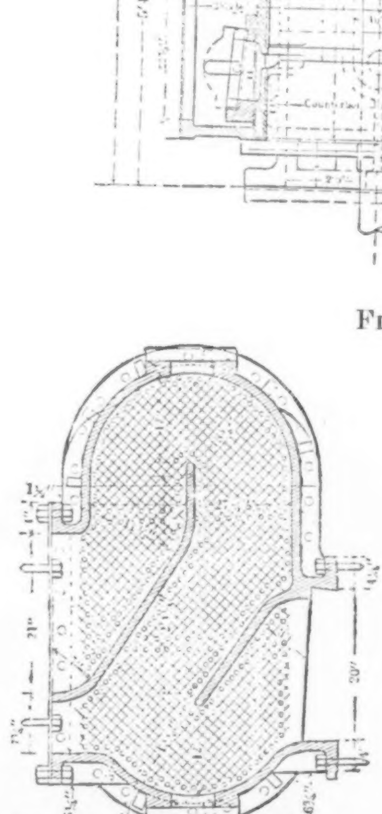


FIG. 34.—CROSS SECTION OF RE-HEATER

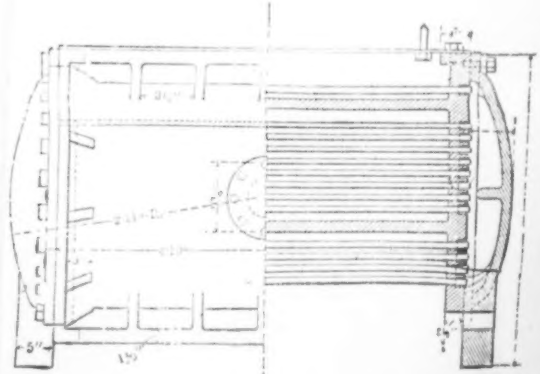
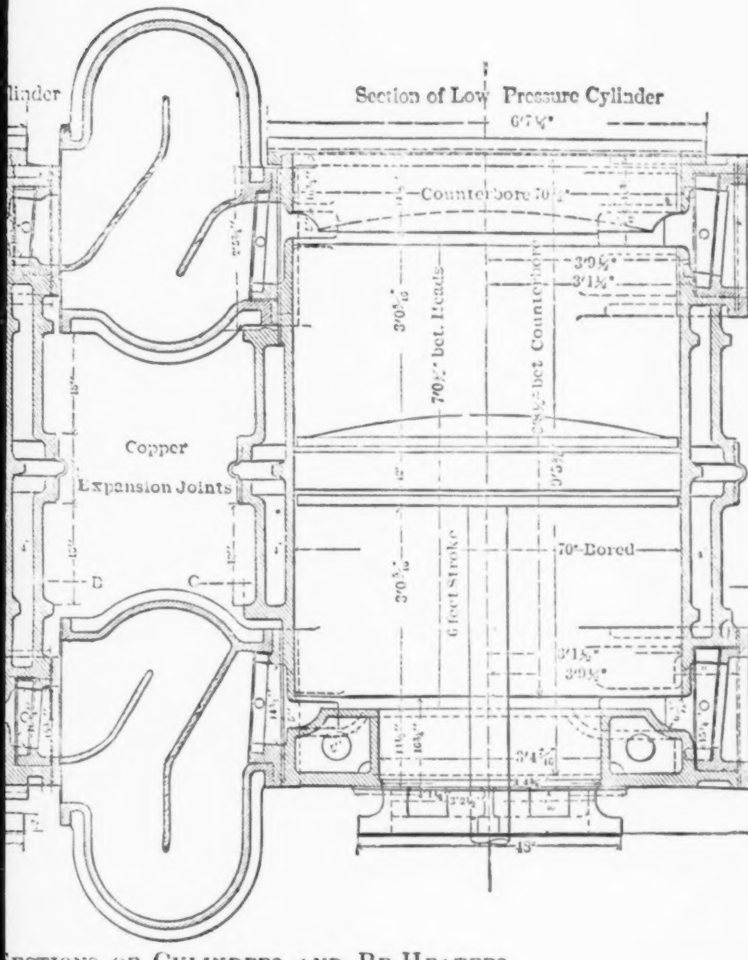
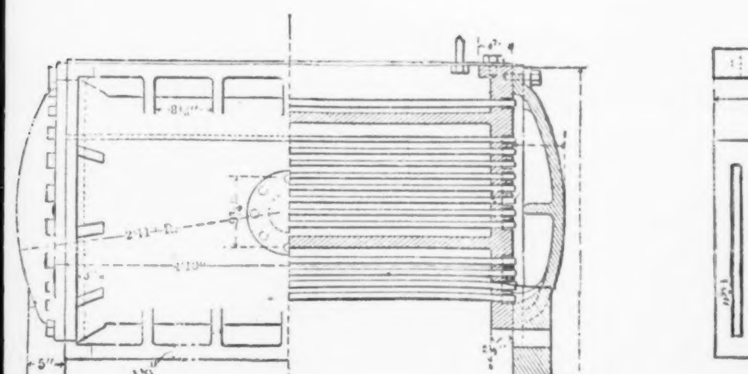


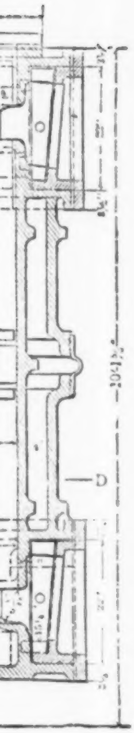
FIG. 35.—LONGITUDINAL HALF SECTION OF RE-HEATER.

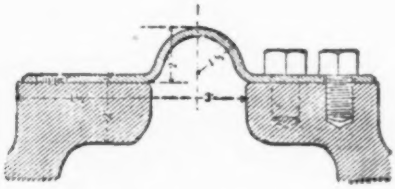


SECTIONS OF CYLINDERS AND RE-HEATERS.

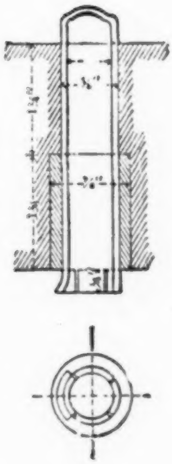


. 35.—LONGITUDINAL HALF SECTION OF RE-HEATER.





Expansion Joint.
Fig. 37.



Tube Packing.
Fig. 38.

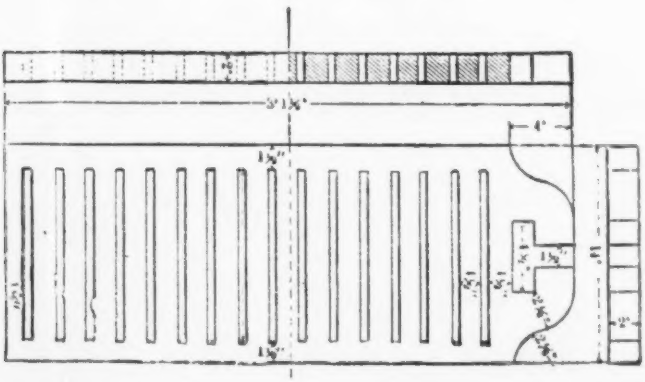


Fig. 36.—Slide Valve.

cases a good deal of wear. I have never broken but one shaft out of several hundred, and then it was after being used two or three years and subjected to a great deal of jarring. I have not tested any for strength and elasticity, but I have found them in practical use about the same as wrought iron, as near as we could judge, not as good as forged steel, but fully as good as wrought iron. I have found it peculiarly good for rollers running against cams where the pressure was on the line of contact, and found them good in journal bearings. They seem to have less tendency to cut than wrought iron. In hardening, they are not very reliable; they are apt to have hidden cracks, but using them soft, I should recommend them for compressive and deflective strains. Perhaps where human life and safety depended upon them it would not be well to use them for tensile strains without further experiment.

MR. A. DAVIS: Before closing the question of this steam-engine, as it is very essential that we should know something about the superheating of steam in its different details, I should like to ask what was the distance of the boilers from the cylinders, and if extra means were used or extra implements employed for lubricating the engine while using these superheaters.

MR. LEAVITT: The main boilers are seventy-five feet from the engine. The design is to put a small boiler close to the engine to be used for reheating, and extra pains have been taken for lubricating the engine throughout. I suppose there has been as much thought bestowed upon that matter as anything else in connection with the design and perhaps more,—to have all the lubrication of all the parts—cylinders, valves, etc., thorough.

MR. A. DAVIS: I would like to ask what were the conditions of the lubrication, as we know that superheated steam has been the cause of great distress in cylinders. I would like to know whether there was anything especially used beyond ordinary lubrication of the cylinder, that we might learn something of some value for our future guidance.

MR. LEAVITT: It is not expected that the steam which enters the cylinders will be highly superheated. It will be the ordinary boiler steam admitted there; the superheated steam will be passed through the tubes of the re-heaters, drying up the moisture. But the main function of the re-heaters is to dry up the moisture that is formed in the mass of the steam by the performance of work in the high-pressure cylinder. We have found valvoline oils to answer very well for high pressure. I have

never had any direct experience with highly superheated steam in cylinders.

MR. EMERY: I would suggest that the point the gentleman raised is met by the fact that the temperature in the low-pressure cylinder can never equal that in the high.

MR. HOLLEY: With reference to the materials, it may be a source of regret to Americans, and ought to be, perhaps, a source of mortification to American steel-makers, that when such large constructions as these thirty-ton shafts are required, that we have to go abroad for them. We have not any hammers in this country adequate to the production of such large shafts. I am not quite sure of that, in view of the development of steel castings. I think the time has come—certainly it is coming—when large shafting, and large forms of structural steel, in every shape, can be more safely made from the metal in the fluid state, and much more cheaply made, than by trusting to the forger. We know that a great many physical defects are developed by the hammer; and last summer I had occasion, at the steel works of Scotland, in Glasgow, to notice a forged steel reversing-engine shaft, of pretty large dimensions, which had been broken. A new one was on hand, so that it did not stop the works, but a steel casting had been ordered for the next one. The people of those works would rather trust the enormous strains of a reversing rolling-mill engine to a steel casting than to a steel forging. If it is in order, I will just say one word about this matter of improved steel castings. It is now more than twelve years since the Terrenoire Company, in France, began experiments, which cost them certainly several hundred thousand dollars, to improve this important art. I will not go on into the details, but mention that, although the manipulations are rather complex, the general idea is pretty simple. Steel castings have been made for a long time, and when they were full enough of silicon and carbon they were sound. There was no trouble in getting them sound enough; but when they were sound they were never strong, and when they were strong they were never sound. But that is a problem which the Terrenoire people tackled, and they tackled it successfully. But you know a very simple result is sometimes got out by very complex and expensive experimenting. The carbonic oxide is undoubtedly reduced by silicon and makes a sound casting; but then the trouble begins with reference to strength. The silica which is then formed remains diffused throughout the whole mass, and

it cannot be strong. There is also a large amount of oxide of iron diffused throughout the mass, which also tends to make it weak. Now the silica that is left by the decomposition of the carbonic acid by silicon is removed by fluxing it with manganese. The silica is so viscous that it will not flow out, but by mixing a suitable quantity of manganese with it it will separate itself from the mass of steel by difference of gravity, and also by that means keeping the steel washed free from this impurity of silica and also from oxide of iron.

Not being a chemist, particularly, I will not attempt to go into these chemical phenomena. Now, as to the results obtained; I have the results of a very large number of experiments, not only made at Terrenoire, but at other places where this same process is used; and, in a general way, I can give you an average result of the composition and the physical character of the very hard steel and the pretty soft steel; the soft steel being more used for structural purposes. The average elastic limit of the hard steel is 60,500 pounds, with a breaking tenacity of 126,000 pounds; that giving only a stretch of from 3 to 6 per cent. Of course that is very hard steel, unsuitable for structural purposes; but a soft steel is made with an elastic limit of 30,200 to 31,000 pounds, and 90,000 pounds breaking strength. In the hard steel the elastic limit is about one-half the breaking strength; in the soft steel it is only about one-third. The same proportions are not reached in steel castings as in steel forgings. That means only that in the present state of the art we must use a little more material, which we certainly can do, because we get it more cheaply. The carbon in the hard steel is, on an average, about one per cent.; silicon is about one-half per cent.; manganese is about one per cent. In very soft steel the carbon runs down to about twenty per cent. The silicon remains from one-tenth to a quarter of one per cent., which accounts for the low elastic limit got out of the high breaking strain; and the manganese is about one-half per cent. So much has been done towards reducing the silicon that I have no doubt that more can be done; now the product of any number of open-hearth furnaces can be very readily run in moulds of any size and shape. They must not be too complex so that the casting will pull apart; or if they are complex, the casting must be yielding. These castings, of any size, may be very cheaply made. They only have to be annealed, or, for certain purposes, they have to be hardened in oil. They are then trussed up so that all the risks of cracks and physical defects in the forgings may be avoided.

XXXI.

A BRIEF TREATISE ON THE STEAMBOAT CAM.

BY LEWIS JOHNSON.

CAMS are very largely, if not exclusively, used in operating the steam and exhaust valves of the engines on our river steamers of the West and South. Although familiar to marine engineers in these localities, cams possess features not so well understood by many others of the profession. It is possible that this article may be of value to this class.

CLASSIFICATION.

Cams are of three different types:

1°. The full-stroke cam;
2°. The cut-off cam;
3°. The folding cam;

I propose in this paper to consider only Types 1 and 2.

SPECIAL FEATURES.

A full-stroke cam in operation has the effect of opening the valves abruptly at the beginning of the engine stroke; maintaining a uniform opening nearly the entire length of stroke, and as abruptly closing the valves at termination of stroke.

A cut-off cam in operation has the same effect as the full-stroke as to opening and closing valves, but maintains the opening only for a given length of stroke, ranging from half to full stroke.

Within reasonable limits both full-stroke and cut-off cams are capable of giving any amount of travel to valves, and while the dimensions differ according to travel, etc., the same rules are applicable for laying off cams of all dimensions.

Any enlargement in the dimensions of cams involves a corresponding enlargement of the yokes embracing the cams; but the same yoke will embrace cut-off cams that differ only in the limit of their cut-off.

Cams are always made of cast iron, and sectional or in halves. for convenience of renewal when worn out or in case of breakage. They are sometimes, but not always, bored to suit the crank shaft, occasionally being cast of approximately proper size in this respect.

The working faces of cams are never finished by other than cleaning off lumps and sand, by rough filing or with the grindstone.

GEOMETRICAL CONFORMATION OF CAMS.

By reference to Fig. 40, the lines embracing a full-stroke cam may be seen. It will be observed that Fig. 40 is inclosed by four curved lines, *P*, *P*, *K* 1, and *K* 2. The position of centre of crank shaft in this irregularly curved body is at *X*. The curved lines *K* 1 and *K* 2 differ in radius, but are drawn from the same point *X*, and hence are concentric with the crank shaft.

The curved lines *P*, *P*, are of like radius, but are drawn from the opposite points *S*, *S*, shown at the intersection of the curved lines *P*, *P*, with the curved line *K* 1. These lines *P*, *P*, are eccentric to the crank shaft.

Figs. 47, 49, 52, and 54, show the lines embracing cut-off cams of given limit, varying from one-half to seven-eighths stroke, viz., one-half, five-eighths, three-quarters, and seven-eighths. It will be observed that the lines are more numerous than those embracing a full-stroke cam, eight being required to inclose a cut-off cam, except the half stroke; as in the figure of a full-stroke cam, *X* represents centre of crank shaft. In Figs. 47, 49, 52, and 54, like letters represent like lines, and lines designated by like letters have the same radius. Let us take Fig. 49 for illustration. Four curved lines are concentric with the shaft, being drawn from the centre *X*, and the remaining four are eccentric to the shaft. The lines concentric to the shaft are *K* 1, *K* 2, of different radius; and the lines *E* 1, *E* 2, of like radius.

The lines eccentric to the shaft are *H*, *H*, *P*, *P*, all drawn with the same radius, but from different points.

APPLICATION OF THE LINES TO LAYING OFF CAMS.

As before stated, Fig. 40 represents the lines embracing a full-stroke cam of given dimensions, which are assumed to be as follows, viz.:

Diameter of crank shaft, 7½ inches; travel of cam, 3 inches; width of yoke, 18 inches.

With one point of the dividers at *X*, the centre of the crank shaft, draw the circle *E* equal to width of yoke, 18 inches. Through this centre *X*, draw the two right lines *A* and *B*. On the line *B*, at the intersection of the curved line *E*, draw the two vertical lines *A* 1, *A* 1. With a radius of 10½ inches, and one point of the dividers at *X*, draw the curved line *K* 1. With a radius of 7 inches, and one point of the dividers at *X*, draw the

curved line *K* 2. With a radius of 18 inches, and one point of the dividers at the intersection of the curved line *E*, with the vertical line *A* 1 at *S*, draw the curved line *P* opposite to *S*, and let it merge or lose itself in the curved line *K* 2. Draw the other curved line *P* from the other point *S*, and we have a full-stroke cam of the dimensions required, which is shown complete at Fig. 41.

In Fig. 42 is presented a view of a full-stroke cam, as usually constructed.

The engravings, including Fig. 46 and upwards, illustrate the lines embracing cut-off cams of varying limits of cut-off, but all of like travel and dimensions, which are the same as those given for the full-stroke cam.

In laying off cut-off cams, the stroke of the engine plays a part in determining their conformation, and in the examples shown this is assumed to be 4 feet. Fig. 46 illustrates the manner of finding essential points in laying off cut-off cams. With *X* as a centre on Fig. 46, and a radius of two feet, draw the circle *E* 1, showing the path of the crank-pin in making a revolution. This circle has a diameter of 4 feet, equal to the stroke of the engine. Draw the horizontal line *B*, passing through the centre of circle *E* 1. Within the limits of circle *E* 1, subdivide the line *B* into eight equal parts, as at 1, 2, 3, 4, etc. Draw the vertical lines, 1, 2, 3, 4, etc., until they each intersect circle *E* 1.

With *X* as a centre, draw the circle *E*, having a diameter of 18 inches, equal to the space in the yoke embracing the cam.

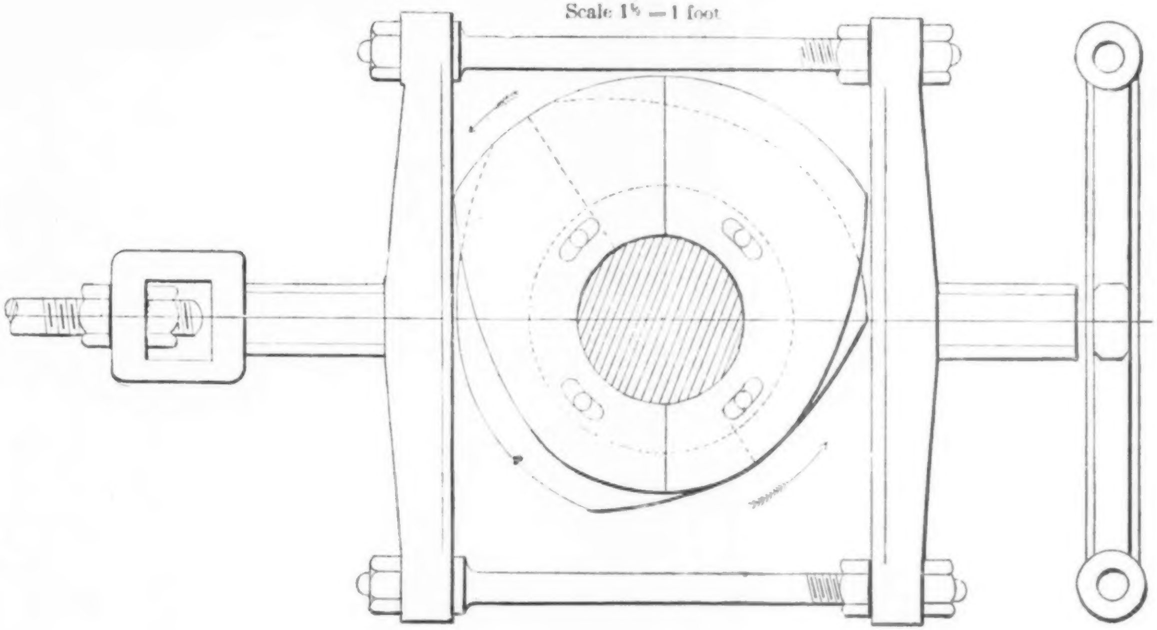
From the centre *X* draw the series of diagonal lines through the points of intersection of the vertical lines 1, 2, 3, 4, etc., from the circle *E* 1, and terminating at *X*. We will now proceed to utilize the scale afforded by Fig. 46, in laying off the cut-off cam shown in Figs. 47, 49, of half stroke limit.

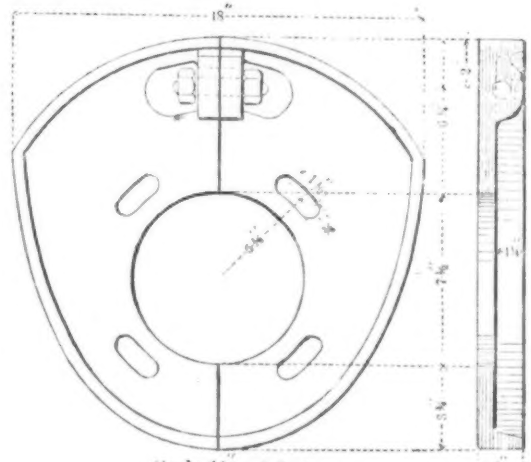
With *X* as a centre, draw the circle *E*, having a diameter of 18 inches. Bisect this circle with the straight lines *A* and *B*, bearing the same relation to their inclosing circle that the lines *A*, *B*, do to the circle *E* in Fig. 46.

It will be observed, in Fig. 46, that the vertical line *A* is also No. 4, representing $\frac{4}{8}$, or half of the stroke. With a radius of 18 inches, and one point of the dividers placed at *V*, the intersection of the circle *E* with the horizontal line *B* in Fig. 47, draw the opposite curved line *P*. With the same radius, and one point of the dividers at *V*, the intersection of the circle *E* with the vertical

FIG. 39.

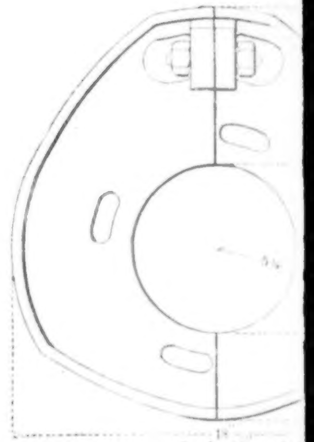
Scale 1½ = 1 foot





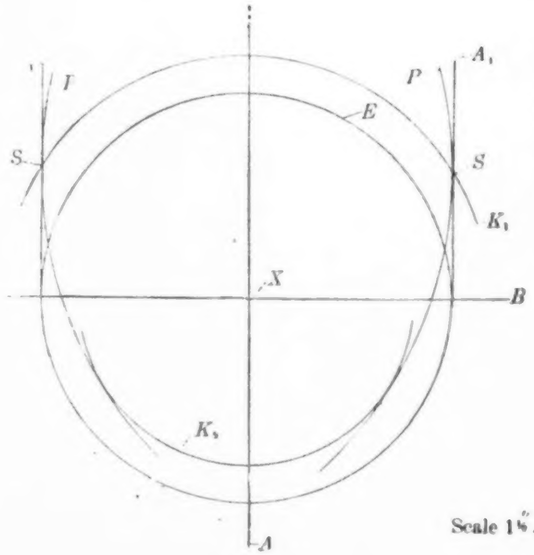
Scale 1½" = 1 foot

FIG. 42.



Scale 1½ = 1

FIG. 43.



Scale 1½" = 1 foot

FIG. 40.

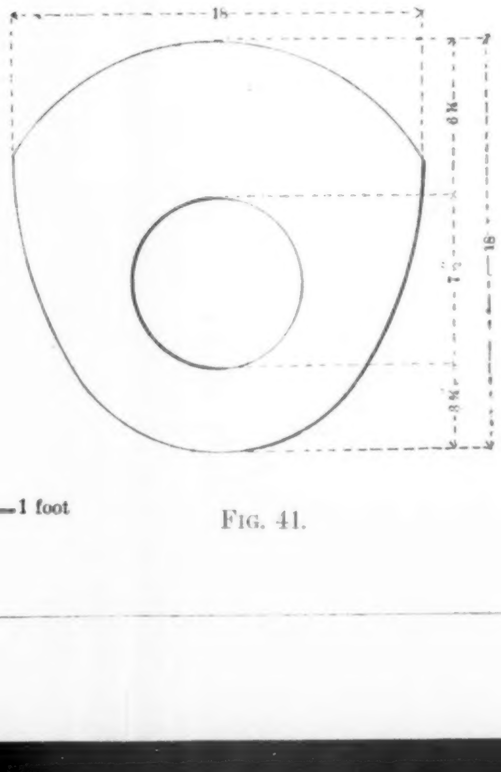
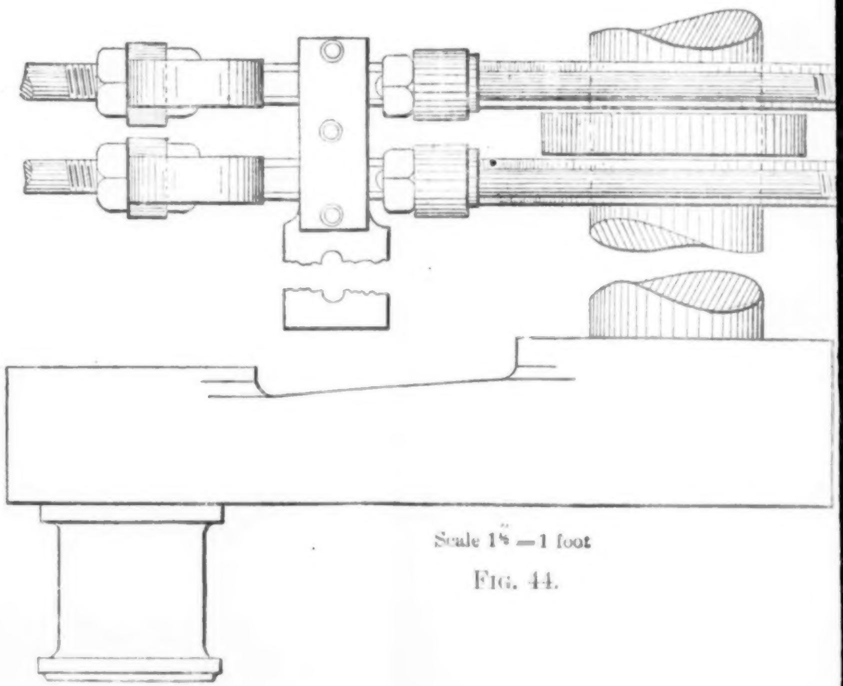
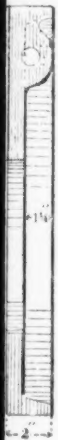


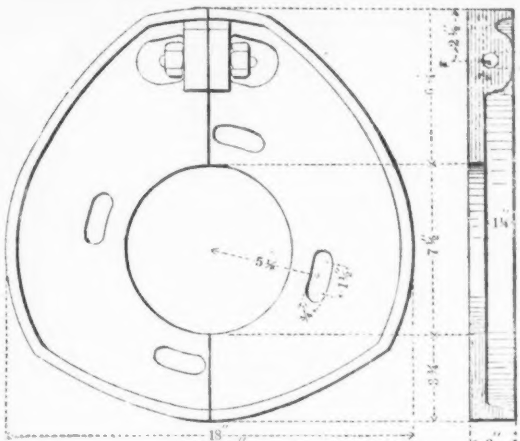
FIG. 41.



Scale 1½" = 1 foot

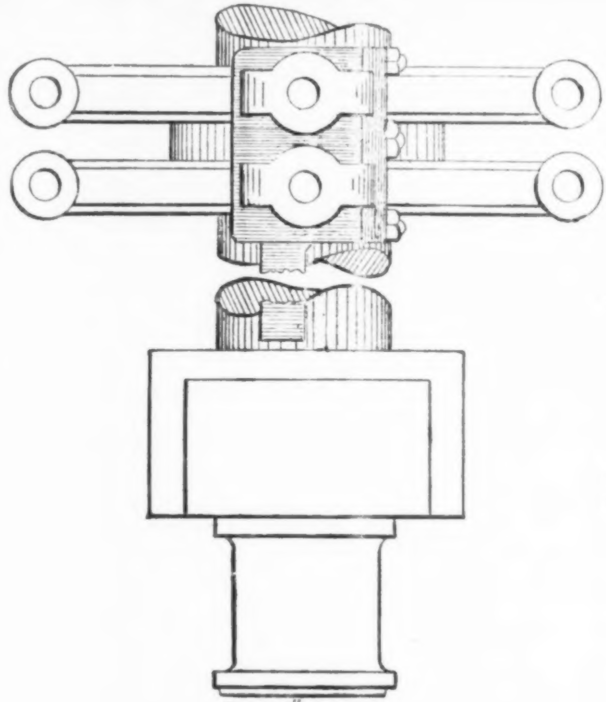
FIG. 44.





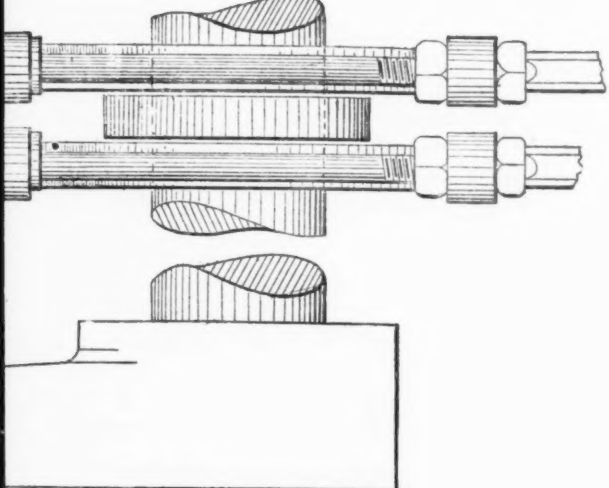
Scale 1½″ = 1 foot

FIG. 43.



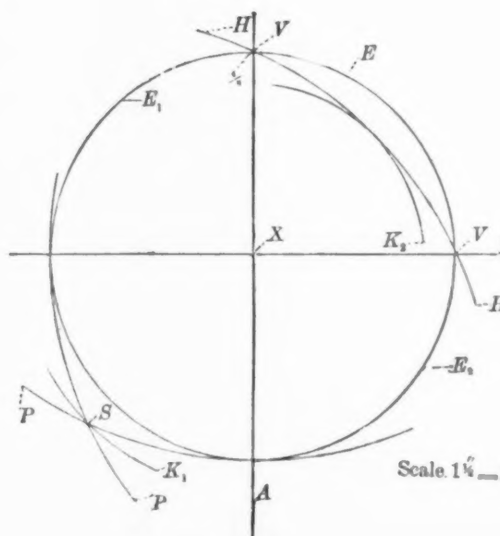
Scale 1½″ = 1 foot

FIG. 45.



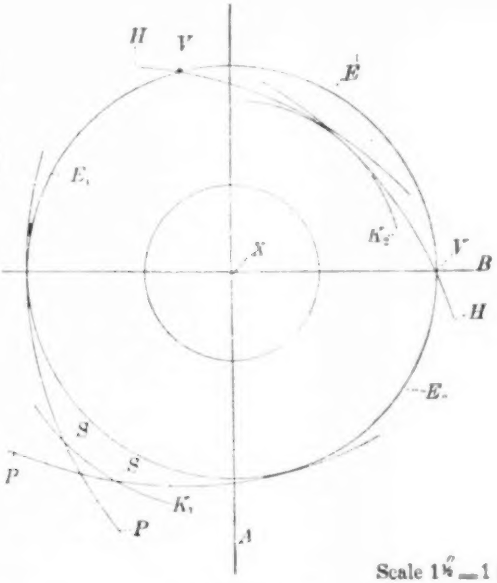
Scale 1½″ = 1 foot

FIG. 44.



Scale 1½″ = 1 foot

FIG. 47.



Scale 1½" = 1 foot

FIG. 49.

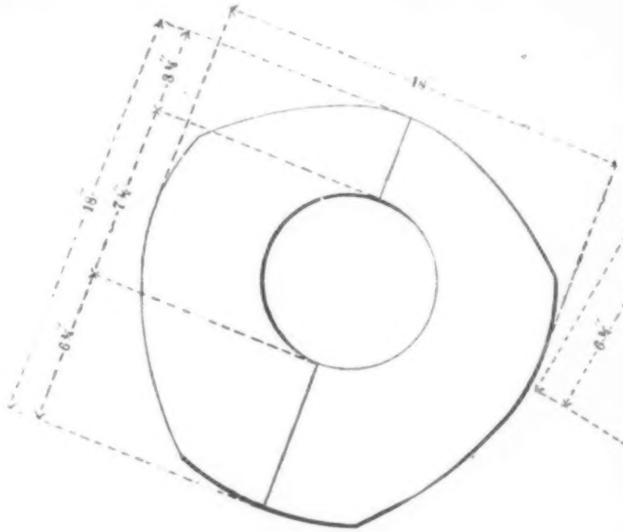


FIG. 53.

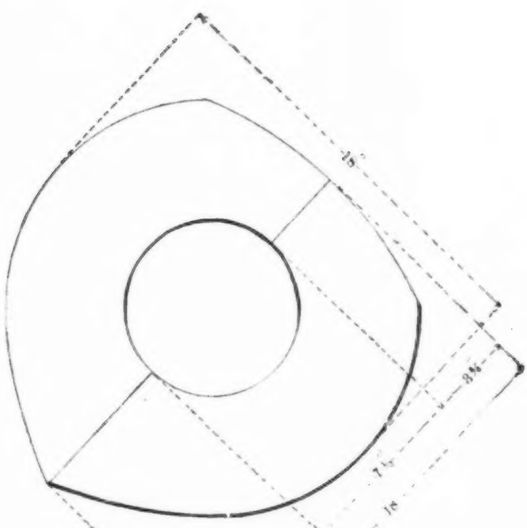


FIG. 48.

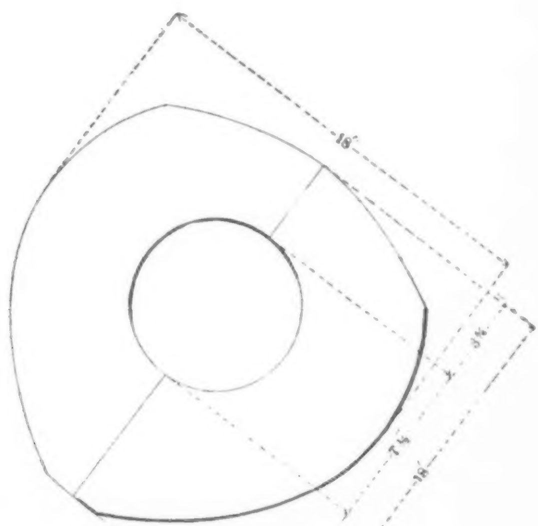
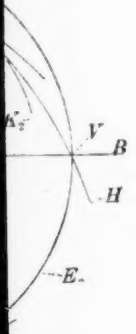


FIG. 50.



Scale 1¼" = 1 foot

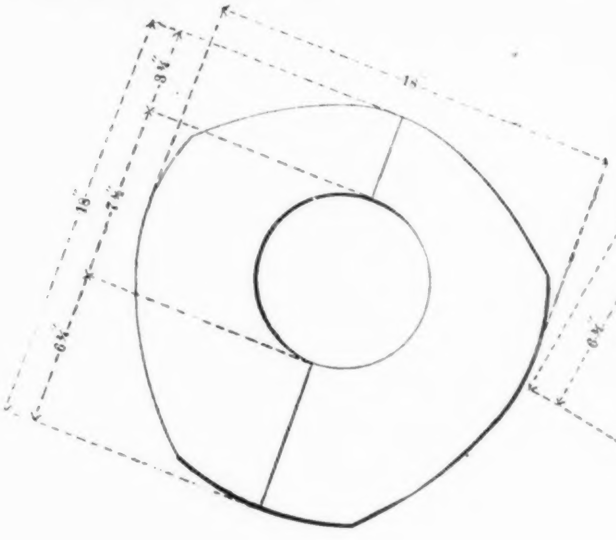


Fig. 53.

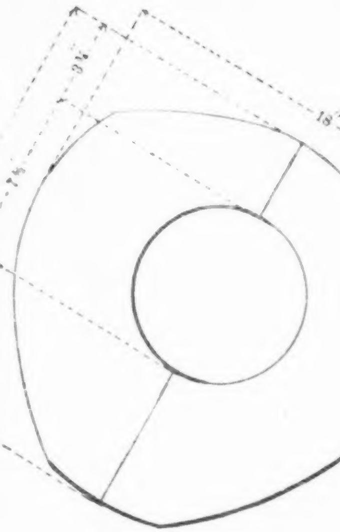


Fig. 51.

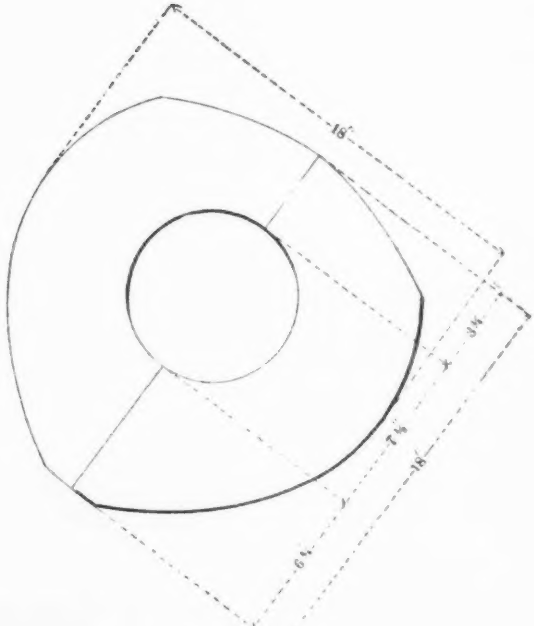


Fig. 50.

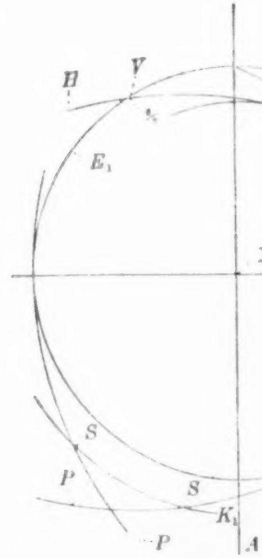
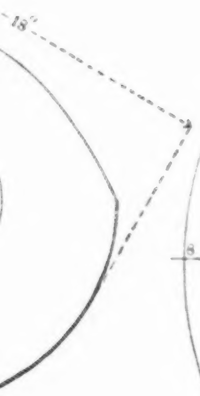
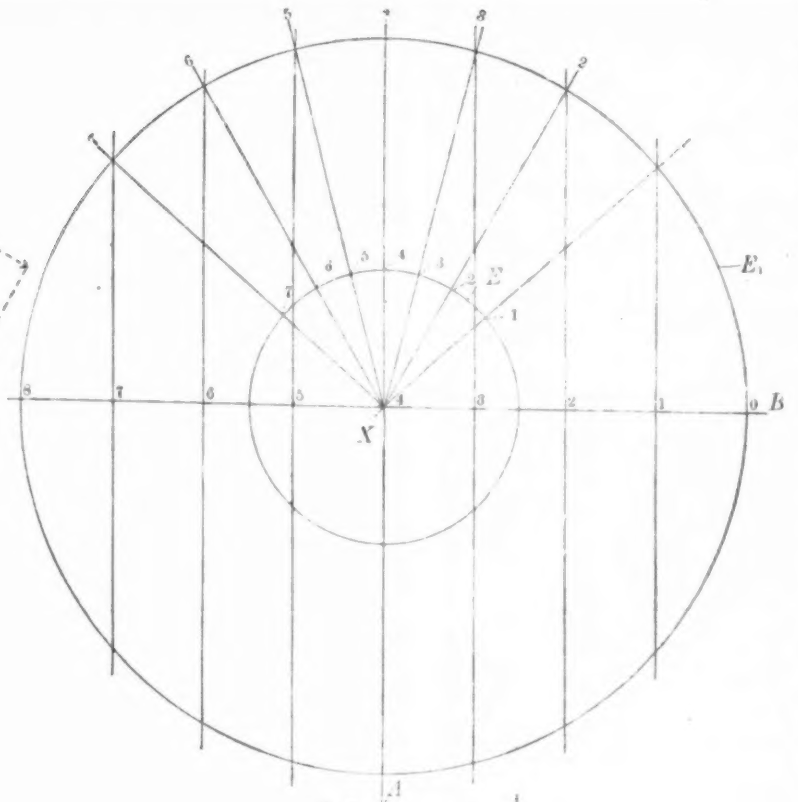


Fig. 52.



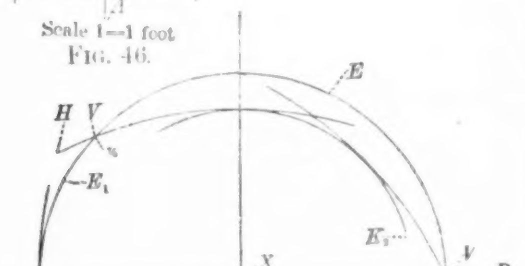


Scale 1"=1 foot

FIG. 46.

Scale 1½"=1 foot

. 52.



Scale 1½"=1 foot

FIG. 54.

line A, draw the opposite curved line P, the two lines P intersecting at the point S. With the same radius, and one point of the dividers at S, draw the opposite curved line H, H. The curved lines K 1 and K 2 are drawn respectively with the radius $10\frac{1}{2}$ inches and $7\frac{1}{2}$ inches, and only serve a half-stroke cam to intersect the curved lines already drawn, as shown in Fig. 47. In practice, the sharp point shown at S would be objectionable, owing to rapid wear at this point, and hence a modification of the dimensions given for this half-stroke cam would be required to obtain a larger wearing surface at the point S, but the cam of this limit is correctly drawn in Fig. 47.

Fig. 48 shows a half-stroke cam complete. Let us now proceed to lay off the five-eighths cut-off cam shown in Fig. 49.

Draw the circle E, and straight lines A and B, as in the preceding example. By reference to Fig. 46 it will be observed that the diagonal line drawn through circle E at 5, is drawn from the straight line marked 5, which intersects circle E 1, and as this straight line 5 represents five-eighths of the stroke laid off on line B, it determines the limit of cut-off on the five-eighths cam shown in Fig. 49.

Referring to Fig. 46, with the dividers measure the distance thereon from the intersection of the vertical line A with the circle E to the intersection of the diagonal line 5 with the circle E, and lay off this distance on the circle E in Fig. 49, to the left of the vertical line A on Fig. 49, as at V.

Now, with a radius of 18 inches, and one point of the dividers fixed at point V, forming the intersection of the circle E with the horizontal line B, draw the opposite curved line P. With the same radius, and one point of the dividers fixed at point V, forming the intersection of the circle E with the point S, taken from the scale in Fig. 46, draw the opposite line P. With a radius of $10\frac{1}{2}$ inches from the centre X, draw the curved line K 1, intersecting lines P, P, at S, S. With a radius of $7\frac{1}{2}$ inches, draw the curved line K 2, opposite to curved line K 1. Now, with a radius of 18 inches, and one point of the dividers fixed alternately at S, S, draw the opposite lines H, H, from their intersection with the circle E, until they merge into the curved line K 2. These curved lines embrace a cut-off cam of five-eighths limit, shown complete in Fig. 50.

From the instructions already given, it should be easy to understand that the three-fourths and seven-eighths cams, shown in

Figs. 51, 52, 53, and 54, are drawn by taking the points of their cut-off from the scale as shown in Fig. 46, at the diagonal points 6 and 7, intersecting circle *E* in Fig. 46; and cut-off cams of intermediate limit of cut-off can be drawn by further subdividing the stroke line *B*, in Fig. 46, into the required limits.

Fig. 43 shows a three-fourths cut-off cam, as usually constructed.

Cut-off cams of any limit are necessarily imperfect in their operations as to uniformity of cut-off from opposite ends of the slides, not from any defect in the rule for laying them off, but from the well-known fact of the crank pin travelling a greater distance, while driven by the piston from the centre of the cylinder, through its curved path from the cylinder, over its centre, and back to the centre of the cylinder, than in accomplishing the remaining distance of its path in making a complete revolution; and, although the subdivisions of eighths of the stroke line *B*, in Fig. 46, do not truly represent a like division of the piston stroke, owing to deviation, caused by inclination of the connecting rod in traversing from the centres to half stroke; still it will be found, that laying off a cut-off cam by this rule, is more nearly correct, than if the divisions on stroke line *B*, were made to correspond exactly with a subdivision of piston stroke into eighths.

The cut-off in cams laid off by the rules herein described, is greater in travelling from one side of the slides, than in travelling from the opposite end, one cut-off being more than the actual cut-off of piston stroke, and the other less; and in practical use, owing to play or lost motion in the connections from cam to valve, the actual cut-off is less than the theoretical; hence cut-off cams are usually laid off to compensate for lost motion; that is, laid off with more limit; for instance, a five-eighths cam would be laid off to cut-off at eleven-sixteenths instead of five-eighths.

PRACTICAL CONSTRUCTION OF CAMS, AND THEIR APPLICATION.

In Figs. 42 and 43 are shown a full-stroke and three-fourths cut-off cam, as usually constructed; and the figures show cams conforming to the dimensions already given, with a width of working face of two inches. Both cams are cast in sections, as shown, being bolted together at one end only; usually with one screw bolt. The four oblong bolt-holes, shown in the figures near the bore of the cam, serve for the passage of screw bolts, which pass through a fast collar on the crank-shaft, one cam being placed on each side of the collar, and the bolt-holes in the collar being

round, and the bolts neatly fitting the holes. By means of these bolts the cams are held securely to the collar, and the relative positions of the full-stroke and cut-off cam to the crank is shown in Fig. 39. The crank is assumed to be on the centre nearest to the cylinder, and revolving in the direction shown by the arrows.

The cams are arranged so that their flanged sides are opposite to each other, their plain faces being contiguous to the collar on the shaft. It will be observed that the oblong holes in the cut-off cam do not bear the same relative position to the sectional line on this cam that the holes do to the sectional line on the full-stroke cam. By reference to Fig. 39, it will be seen that the round holes, shown in the centre of the oblong holes near the shaft, are equidistant from themselves, and from the sectional line on the full-stroke cam; these round holes are those in the fast collar on the shaft; and the reason for making the bolt-holes oblong in the cams, is to allow of angular advance or adjustment for securing lead, or *vice versa*. From observation of the cut-off cam, it is clear that if the sectional line of this cam were parallel with the sectional line of the full-stroke cam, both lines being at right angles to the crank, the concentric curved line of the cut-off cam would be in juxtaposition to the face of the yoke; and that no rectilinear movement of the yoke would ensue, by rotation of the cam, until the eccentric curved line of the cam had reached the face of the yoke. As it is necessary that the movement of the yoke should be coincident with that of the crank in passing its centre, the cut-off cam is, therefore, projected forward, until it assumes the position shown in Fig. 39; and hence the bolt-holes are located on the cam, to admit of this lead or angular advance.

On a double crank shaft, cut-off cams become right and left, as to the location of these bolt-holes; and, while in every other respect the two cut-off cams are essentially the same, and made from one pattern, the bolt-holes in one are as much on one side of the sectional line, as they are on the opposite side of this line on the other cam.

The angular advance, shown to the cut-off cam in Fig. 39, is proper only when the shaft rotates in the direction shown by the arrows; and if the direction of rotation were reversed, a corresponding advance of the cut-off cam would be required in the opposite direction. All cut-off cams, as ordinarily applied, are arranged only for effect in moving one way, as will be clear by observing their construction.

Fig. 39 shows the full-stroke cam and cut-off cam, both ready to impart motion to the yoke at the instant of the crank crossing the centre; but no lead is given to either; and as it is customary, in our high-pressure engines, to have the opening of the valves lead the crank, this is accomplished in a homely but efficient way by the interposition of blocks between the lifter and the levers attached to the valves.

The yokes, shown from different views, at Figs. 39, 44, and 45, are substantially constructed as shown; and are supported by brackets at each end, securely bolted to the pillow block.

In the engravings, this bracket is shown only applied to one end.

This article has by no means exhausted the profitable consideration of this subject, and those interested may, with the instructions already given, either with additional drawings or models, still further investigate the operation of cams, as to their position relative to the crank and piston in accomplishing an entire revolution.

XXXII.

ON THE RATIO OF EXPANSION AT MAXIMUM EFFICIENCY.

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In all heat engines the method of transformation of heat-energy into useful mechanical work is the following:

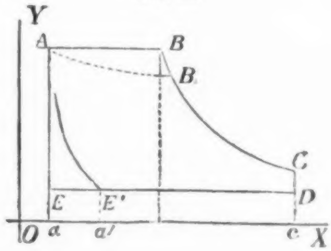
A certain mass of the working fluid is heated from a temperature which is usually not far from that of the atmosphere up to some higher temperature, T. This is accompanied by a definite increase of volume, or of pressure, or of both, and in the case of liquids by a change of physical state after passing a certain point which is variable, but definite for each pressure; this latter temperature is the boiling-point, and the change is that known as vaporization. Evaporation being complete, the mass is expanded until it has attained a certain larger volume, v_2, the magnitude of which is r times that of the initial volume, v_1, with which expansion began. We thus have the "*ratio of expansion:*" $r = \frac{v_2}{v_1}$.

When an expansion is complete, the whole volume, v_2, of steam or gas at the pressure p_2, is rejected from the cylinder into a con-

denser or into the atmosphere, and the piston which it has impelled through the total volume, v_2, returns to the starting-point, resisted by the "*back*-pressure," p_3, of the condenser, or of the atmosphere. During the latter operation all heat which has not been transformed into work is rejected, and an additional amount is expended, which is equivalent to the work done by the piston upon the fluid during its expulsion. This operation is also similar in result to the following: Instead of exhausting the working fluid as described, abstract heat from it at the maximum volume, v_2 until its pressure becomes equal to the back-pressure; then compress at the constant pressure p_3, until the fluid is restored to its original volume at the pressure p_3, which volume, in the case of the steam-engine, may be neglected. In the case of the gas engine or hot air engine this volume is that of the working fluid at initial temperature and pressure.

This process is thus graphically represented: In Figure 55, the

FIG. 55.



fluid, initially in the state measured by the pressure $a\ E$ or $a'\ E'$ and volume $O\ a$ or $O\ a'$, is heated, sometimes at constant volume, as $O\ a$, and sometimes with compression, as from $O\ a'$ to a higher temperature, the pressure and volume varying as shown by $E\ A$ or by $E'\ A$. Heated next at constant pressure or at constant temperature, the mass expands, doing work, to B or to B'. At this point v_1, p_1, the supply of heat ceases, and the fluid expands "adiabatically," transforming into mechanical energy all the heat demanded as equivalent to the work measured by the area $b\ B\ c\ C$, and drawing upon its own stock of heat to supply this demand. At the end of this stage the fluid has a lower temperature and a pressure and a volume, $c\ C$, $O\ c$ (p_2, v_2), determined by that temperature and the value of $r = \frac{v_2}{v_1}$, and which are indicated by the location of the point C.

Rejecting heat at constant volume, v_2, pressure falls to D, p_3, and then rejection of heat continuing at constant pressure, p_3, the volume is reduced to that with which it started.

The *total* or *gross* work done is in gas engines, measured by the area $A\ B\ C\ c\ a\ A$, in steam and vapor engines by this area increased by a very considerable amount—the measure of internal, of molecular, work which cannot appear on the indicator diagram.

The net work done is measured by the area included in the indicator diagram, $A\ B\ C\ D\ E\ A$. This work is the equivalent of all heat transformed into mechanical work or energy. *The efficiency of the fluid* is the ratio of *net* work done to total heat received by the fluid, and is a maximum when the area $A\ B\ C\ D\ E$ is a maximum, assuming the *rate of expansion* alone to vary. It is evident that this maximum is determined, therefore, by the conditions which make the area $b\ B\ c\ C$ a maximum, which conditions are very simple in the hot air engine, and are easily expressed, while in the steam and in vapor engines they are very difficult of determination and expression in consequence of their extreme variability.

But the efficiency of the fluid is but one factor in the determination of the rate of expansion for maximum economy. The heat in the fluid is compelled to do its work, not simply through that fluid as a transmitting mechanism, but also through a machine which, as an apparatus intended to imprison and direct so subtle and elusive a form of energy as heat, is extremely imperfect, and which has the additional and very serious defect of being itself cumbersome and difficult to start and to keep in motion without considerable loss of power within itself.

The useful work of the machine is that which it transmits beyond its own boundaries to other mechanisms, and this is a maximum at that rate of expansion which gives energy to the machinery of transmission beyond the engine at least cost in heat expended. This *efficiency of the system* is therefore the product of two factors, the *efficiency of the fluid* and the *efficiency of the engine* considered as a piece of mechanism.

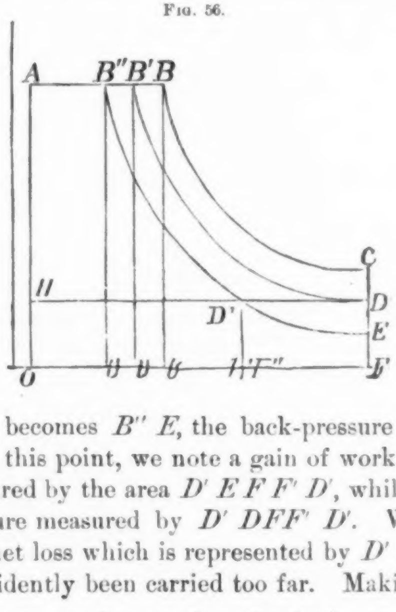
(1.) Taking first the purely ideal case in which the mechanism is assumed to be perfect and the rate of expansion the only variable element, we may, by examining Figure 56, see at once what should be the value of that rate.

It is obvious that the rate of expansion simply determines how far the transformation of *stored* heat-energy existing at B shall

be continued by the development of work during the expansion of the working fluid. It is equally obvious that this expansion shall continue until the gain of work by further expansion is more than balanced by losses avoidable by termination of that process.

First: Where the only loss is due to a fixed back-pressure, $FD = p_3$, it is seen that, were expansion to cease at C, the work which would have been done had the expansion line $B\,C$ extended to the right, beyond C is lost, and that the counterwork of back pressure beyond that point is gained; but the former exceeds the latter, and the net result is a loss by incomplete expansion. On the other hand, were the rate of expansion increased so that the

FIG. 56.



expansion line becomes $B''\,E$, the back-pressure line is reached at D'; beyond this point, we note a gain of work done usefully, which is measured by the area $D'\,E\,F\,F'\,D'$, while a loss accrues by back-pressure measured by $D'\,DFF'\,D'$. We thus again meet with a net loss which is represented by $D'\,DED'$, and expansion has evidently been carried too far. Making the value of $r = \frac{v_2}{v_1}$ such that expansion reaches the back-pressure line at D and p_2 becomes equal to p_3, we meet with neither kind of loss, and it follows that expansion should, in this ideal case, be continued until the expansion line meets the back-pressure line.

This may be readily shown by other methods. It was shown, nearly two generations ago, by Sadi Carnot, that maximum efficiency of *fluid* is attained when expanding between the widest possible limits of temperature.

It is now well known and it is shown by every elementary treatise on physics, or mechanics, or thermodynamics, and on heat-

engines, that the efficiency of the *fluid* in any heat-engines is measured by the expression: $\frac{T_1 - T_2}{T_1}$, in which T_1 and T_2 are the temperatures of reception and rejection of heat measured from the "absolute" zero. But this maximum range of temperature corresponds to the maximum attainable range of pressure, and, the upper limit being fixed, this range is determined by the value of and is a maximum, when $p_2 = p_3$ and expansion continues to the back-pressure line.

A general analytical demonstration is obtained in the following manner: Given p_1, v_1, v_2, p_3, to find the value of *rate of expansion* r to make the net work done a maximum. This work, *A B C D E*, Figure 55, is measured by

$$W_a = p_1v_1 + \int_{v_1}^{v_2} p \; dv - p_3v_2 \; ; \quad . \quad . \quad . \quad . \quad . \quad (1)$$

it is a maximum when the variable part $\int_{v_1}^{v_2} p \; dv - p_3v_2$ is a maximum.

The method of variation of p with variation of v is determined by various conditions, which need not be discussed here and which do not affect our analysis. Let this relation be such that we may write, as experiment indicates that we may do with practically close approximation,

$$p_1v_1^n = p_1v_2^n.$$

Thus we have

$$W_n = p_1v_1 + \int_{v}^{v_2} p \; dv - p_3v_2 \; ;$$

$$= p_1v_1 + \frac{p_1v_1 - p_2v_2}{n - 1} - p_3v_2 \; ; \quad . \quad . \quad . \quad . \quad . \quad (2)$$

or where $n = 1$,

$$W_a = p_1v_1 \left(1 + \log_e r\right) - p_3v_2 \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Determining the maximum for the first and usual case, we get

$$\frac{d\,W_n}{d\,r} = d\left(p_1v_1 + \frac{p_1v_1 - p_1v_1r^{1-n}}{n - 1} - p_3\,r\,v_1\right)\frac{1}{dr} = 0\;;$$

whence
$$r = \left(\frac{p_1v_1}{p_3v_1}\right)^{\frac{1}{n}} = \left(\frac{p_1v_1}{p_2v_1}\right)^{\frac{1}{n}} = \frac{v_2}{v_1} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Hence $p_3 = p_2$, and the rate of expansion for maximum effi-

ciency is that which makes the terminal direct pressure equal to the pressure resisting the motion of the piston.

This analysis must be modified when the expansion line is an equilateral hyperbola, in which case we have $n = 1$ and $p_1 v_1 = p_2 v_2$. This case is often assumed in the theory of gas and air engines, as it is that of isothermal expansion; but it is probably rarely observed in actual practice, and perhaps never occurs in steam and vapor engines. In simple calculations of work, however, the assumption does not usually lead to serious error,* and, so expanding the working fluid, the energy exerted by it up to the point of cut-off is equal to the lost work due to back pressure; the net work done is measured by the total area under the expansion line of the indicator diagram, and the efficiency is proportional to log r.

Thus we have

$$W_1 = p_1 v_1 (1 + \log_e r) - p_3 r v_1$$

$$\frac{d W_1}{d r} = 0 = \frac{p_1 v_1}{r} - p_3 v_1$$

$$r = \frac{p_1}{p_3} = \frac{v_2}{v_1} = \frac{p_1}{p_2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

whence we again find $p_2 = p_3$.

(2.) But in all real engines we have a resistance to the motion produced by the expanding fluid, which is composed of two parts, an actual back pressure on the piston $p_b = p_3$, as in the ideal case above, and a resistance due to friction of engine, including its pumps and attachments. It is evident that, as the latter, p_f, like the back pressure, p_b, is a constant source of lost work, we must terminate the expansion as soon as this source of loss produces a greater loss of power or of work than is gained by further expansion. In fact, given a certain value for the sum of these resistances, $R = p_b + p_f$, it is obvious that we may consider the whole as back pressure, if we choose, and that it is a matter of indifference, so far as the determination of the rate of expansion is concerned, what are their individual magnitudes.

To determine $R = p_b + p_f$, the sum of resistances due to back pressure, p_b, and to the frictional and other resistances—as of pumps, etc.,—denoted by p_f, take an indicator card from the engine unloaded. Its mean pressure measures the friction, p_f,

* See a neat demonstration of this case by Professor Marks in the Journal of the Franklin Institute, June, 1880.

of the unloaded engine, and this, increased by a fraction of the pressure added by the load,* is the value of p_f. Or, still better, determine the indicated and the dynamometric power of the engine simultaneously; their difference is lost work, and the value of p_f, corresponding to that work, is that required.

Hence, for actual engines, where no other cause of loss exists to any appreciable extent, as in some types of air engines, we may write

$$W_b = p_1 v_1 + \frac{p_1 v_1 - p_2 v_2}{n - 1} - (p_3 + p_f)\, v_2 \quad . \quad . \quad . \quad . \quad (6)$$

and by the process already outlined we obtain a maximum and deduce

$$p_2 = p_3 + p_f \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Hence: *Where the only variable loss is due to back pressure and to friction of the engine, the rate of expansion should be such as to cause expansion nearly to the mean pressure line of the engine diagram taken without load;* the useful work is, as before, the gross work done during expansion, and, thus adjusted, the net useful work and the efficiency are nearly proportional to log. r.

These conclusions may be taken practically without qualification for all actual engines in which the working fluid is gaseous and subject to little direct loss of heat.

(3.) For steam and other vapor engines still further and still greater modification is necessary, since in such engines the departure from the ideal conditions first assumed is not only greater than in gas and air engines, but is so great as, in most cases, to lead to radically different rates of expansion. Even in the gas engines, the action of the working fluid, as assumed above, is very greatly modified by such variations from the ideal conditions as are here referred to.

These engines—steam and vapor—are impelled by a fluid, which is a vastly better receiver and transmitter of heat than the permanent gases. Steam takes up and loses heat, in the processes of formation and of condensation, with extreme rapidity. The working fluid, in all steam-engines, is readily condensable, and exchanges heat with the metallic surfaces of the working cylinder with the greatest freedom. It is usually more or less

* Experiments made at the Novelty Works for the Navy Department, 1865-67, and others made later, do not exhibit this increase.

wet, and its humidity is subject to rapid and extreme variation in the course of the movement of the piston.

Explosive and other gas engines are impelled by a mixture of hot gaseous and vaporous products of combustion, of which the latter portion is, like the working fluid in the steam and other vapor engines, subject to rapid and considerable changes of thermal state. Inclosed, usually, in a chamber the sides of which are kept cool by a water-jacket, enormous quantities of heat are lost as expansion proceeds, and the efficiency of the machine is correspondingly diminished, and the economical rate of expansion is altered by the increased losses which accompany the higher rates. It is easily seen that, should these losses increase in a high ratio, with large rates of expansion, a point will be reached, and may be reached quickly, at which any greater expansion will result in a loss exceeding in amount the work gained by the extension of the expansion line. This point may be reached, and probably often is reached, long before attaining the limit set by the value of the resistances already studied. In such a case, we may call this limit that of the *virtual back pressure due to condensation*, and may designate it as p_c.

(4.) In the steam engine a still more complicated set of phenomena is to be met with, and the result of their action is similar to that just described.

Suppose steam to enter the steam cylinder perfectly dry, and to expand *adiabatically*.* As expansion progresses, after the closing of the steam valve by the expansion gear, the work done by the working fluid results in the transformation of so much heat into mechanical energy—which heat can now only be obtained by drawing upon the stock contained in the steam itself—that a part of the steam becomes liquefied.

This fact was shown by Rankine and by Clausius, by the study of the thermodynamics of the case; but it can easily and satisfactorily be shown by any student or engineer who will take the trouble to calculate the "total heats" of steam at the pressures found at the beginning and at the end of the expansion line. It will be found that the work done during the expansion is greater than the mechanical equivalent of this difference, and it follows that a part of it must have been done at the expense of the heat of evaporation of the expanding mass. This must be the case up to a limit at which the sensible heat of steam is just equal to

* Without receiving or losing heat by exchange with surrounding surfaces.

the latent heat of the mass when indefinitely expanded at the absolute zero, and is about $\frac{1109550}{772} = 1437^{\circ} \pm$ absolute, a temperature only attained at the red heat.

Steam, therefore, condenses in the steam cylinder unless, by superheating or by the use of an efficient jacket, considerable heat is supplied it during expansion or before. This amount is, however, insignificant in comparison with direct losses of heat; it can probably never approach ten per cent. of the heat supplied, and is more likely, usually, to be a very much smaller figure, perhaps two or two and a half per cent. for average cases.

Initial condensation and later re-evaporation of steam in the steam-engine, and initial condensation without subsequent re-evaporation, in gas engines, give rise to losses that are both absolutely and relatively very great wherever the range of temperature during expansion is very considerable, and especially with low back pressure.

The steam passing out of the exhaust ports to the condenser or into the atmosphere is moist and heavy with the water of condensation, and is a good conductor of heat, as well as a very greedy absorbent. It sweeps out of the cylinder large quantities of heat abstracted from the inner surfaces of the cylinder, leaving those surfaces comparatively cold and wet with a chilling dew. The entering steam meets these cold metallic and liquid masses and is condensed in sufficient quantity to reheat them to the temperature of prime steam. As the piston moves forward it uncovers new surfaces, and condensation continues until sometimes a large fraction of the steam supplied lies in the cylinder or floats in the uncondensed steam as water and mist. Toward the end of expansion, and especially during the exhaust, re-evaporation occurs at lower pressures and to a similarly serious extent. Thus heat is constantly transferred from the steam to the exhaust side, and, doing almost no work, is wasted, and the efficiency of the engine and the cost of fuel are greatly affected. The heat thus lost frequently amounts to 25 per cent. of the total supplied, and has been known not infrequently to amount to 50 per cent. In one case, noted by the writer, initial condensation was as high, *at least*, as 80 per cent.*

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* The extent of the loss from this cause is very seldom realized by engineers, and still less by unpractical writers on the theory of the steam-engine. Even Rankine, the greatest of all known writers, seems to have failed to detect an initial condensation of 26 per cent., shown apparently by the value obtained for

Since loss from this cause has been found to be so great, and to increase so rapidly with increased expansion, that it practically often sets an early limit to the economical increase of the rate of expansion, it is evident that we may determine a point such that, expansion being carried beyond it, the losses due this cause will exceed the gain of work done by the expanding fluid, precisely as in the cases already cited. Measuring the loss at such a point, we may determine the equivalent in foot-pounds of work, and thence deduce the magnitude of a new equivalent "*virtual* back pressure," $p_v = p_c$, which, if actually existing as such pressure, would similarly limit expansion.

This value inserted in the formula representing the work of the steam would give a measure of the rate of expansion of maximum efficiency.

We should have, assuming no other losses:

$$W_c = p_1 v_1 + \frac{p_1 v_1 - p_2 v_2}{n-1} - p_c v_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and should expand until $p_2 = p_c$, and make

$$r = \frac{v_2}{v_1} = \left(\frac{p_1}{p_c}\right)^{\frac{1}{n}}.$$

Thus, for all cases, whether of expansion of steam, of air, or of the products of combustion in explosive gas engines, we may determine for each case a certain "*virtual back pressure*," which we may call p_v, by which to indentify a point beyond which continued expansion leads to a loss of heat, or of work, or of both combined, that is greater than the gain by work done* in that additional expansion, and may write generally:

$$W_v = p_1 v_1 + \frac{p_1 v_1 - p_1 v_1 r^{1-n}}{n-1} - p_1 v_1 r^{1-n} \quad . \quad . \quad . \quad . \quad (9)$$

$$r = \left(\frac{p_1}{p_v}\right)^{1-n}$$

the index in his formula for the adiabatic expansion of steam ($p\, v^{\frac{10}{9}} =$ constant). He seems to check the discrepancy introduced into his analysis, amounting to a loss of about one-fourth of all heat supplied, by underestimating the efficiency of the boilers, crediting them with but 0.54, where a low estimate, in the opinion of the writer, would be 0.65, and a fair figure would be 0.68 or 0.70 for the cases taken. The writer has never known the efficiency given in those estimates to be attained with boilers of such low value.

* See "The Limitations of the Steam-engine," by Professor Marks, Journal of the Franklin Institute, August, 1880. The use of the term "virtual back pressure" is not logically correct, as the two methods of loss are quite different; but the writer has not yet found a more satisfactory term to take its place.

Where, as in the case in which air expands isothermally, the expansion line is an equilateral hyperbola, it is seen that the loss by the virtual back pressure is always equal to the work done in the engine up to the point of cut-off; for $p_1 v_1 = p_v v_2$.

The net work done is always, for such expansion,

$$W_n = p_1 v_1 \log_e r = p_1 v_1 \log_e \frac{p_1}{p_v}.$$

In general, for the *net* work shown on the card as $p_2 = p_v = p_1 r^{-n}$; $p_v v_v = p_1 v_1 r^{1-n}$; $p_v r^n = p_1$, we get for net work:

$$W_n = \frac{n}{n-1} p_1 v_1 \frac{r^{n-1} - 1}{r^{n-1}} = \frac{n}{n-1} p_1 v_1 \left[1 - \left(\frac{p_1}{p_v}\right)^{\frac{1-n}{n}}\right]$$

$$= \frac{n}{n-1} p_1 v_1 \left(1 - \frac{1}{r^{n-1}}\right). \quad . \quad . \quad . \quad . \quad . \quad (10)$$

In the effort to determine the value of $p_v = p_e$ for this last method of loss of efficiency, we meet with great difficulties. The loss from initial condensation and later re-evaporation is the most serious of all those losses which in expansive engines are in any degree due to defects of the machine as a machine, and they are among those which are controllable to a considerable extent by the engineer. No two engines, however, ever exhibit them in the same degree, and modifying conditions are so numerous and so potent that the result of the most painstaking efforts to classify and to formulate them are likely to be exceedingly unsatisfactory.

This loss is proportionally greater as the range of temperature during expansion is greater; it is increased by slow speed of engine,* by reduction of the real back pressure, by increase in size of engine for a given amount of work done, by increase in conductivity of the surfaces of the working cylinder, and especially by wetness of steam. It is reduced by low rates of expansion, by increasing back pressures, by reducing initial pressures, by increasing speed of engine, and by special expedients, as steam-jacketing, superheating, and the division of the expansion between two or more cylinders, as in "compound" or double-

* With speeds so low that the range of temperature of cylinder surfaces is not restricted, the total weight of steam condensed is probably constant, and the loss becomes inversely as the speed of piston, or as the weight of steam passed through the engine. For fairly high and for very high speeds the writer makes this loss proportional to the reciprocal of the square root of the speed. See records of United States expansion experiments.

cylinder engines. Even increasing compression may reduce this loss and thus give a higher steam-line and an altered expansion-line.

The waste becomes the less, when the sides of cylinders only are jacketed, the smaller their diameter; it is lessened, when both heads and sides are jacketed, by increasing diameters, volumes being in both cases equal. With superheated steam, and where there is little initial condensation to be anticipated, the shape of cylinder is determined by the minimum ratio of volume to *internal* superficies, *i.e.*, $\frac{\text{diam.}}{\text{length}} = 1$, except—as is often, if not usually, the case—when it is controlled by commercial considerations. The surfaces of the piston must evidently be included, since the principal losses—those due to initial condensation and to re-evaporation—occur upon those surfaces.

In general, we may say that the efficiency of an engine is some function of Δt, V, P, and A; but the difference of temperature, Δt, is a function of pressures and time of exposure; the speed, V, determines time and exposure, and the area of surface exposed, A, is a function of volume, per unit of weight of steam, and of shape of cylinder. All of the conditions are so involved and interdependent that the simple approximate expressions to be presently given may be found preferable to any exact formula, even were it possible to devise them satisfactorily, and as these simple expressions yield, all things considered, very fair results, we may be fully justified in their use until extended and exact investigations shall yield better.

In gas engines the waste is decreased in those in which the working fluid meets only non-conducting surfaces, while it amounts as a minimum to 60 or 70 per cent. in some slow-running water-jacketed cylinders.

Again, we find some interesting compensations. The difference in back pressure between non-condensing and condensing engines is productive of such a wide difference in the range of temperatures worked through in usual cases, that the writer has been accustomed to consider the compensation so complete as to justify the assumption that the value of *this* "*virtual* back pressure" may be assumed to be independent of the magnitude of the actual back pressure, and to be determined solely by other conditions above noted. In steam-jacketed engines the efficiency of the steam-jacket is reduced by high speed, while the losses that it is designed to check are rendered less by the reduced effect of other causes of variation of the amount

of initial condensation, and, while this condensation is by no means complete, the error introduced by the assumption that it is so, may perhaps be neglected in presence of so many other and such complicated causes of irregularity of action. Our approximation must be anything but close at best. The exact expression would probably involve the Newton law of cooling and values of differences of temperatures, deduced from Rankine's formula : $\log. p = A - \frac{B}{T} - \frac{C}{T^2}$.

The best that the writer has been able to do in this direction, as yet, is to make simple and roughly approximate expressions for values of $p_t = p_v$, the proper terminal pressure, which, while widely departed from in many cases, may fairly represent average practice, and serve as a guide to the designer and engineer until something better can be done, thus :

For the common unjacketed steam-engine take the limit for p_v that determined by back pressure plus friction solely up to pressures of 6 or perhaps 7 atmospheres.

Then the terminal pressure should be, nearly, for non-condensing engines $p_t = p_v = p_b + p_f = 14.7 + 3.3 + 2 = 20$ pounds per sq. inch (1.4 kilos per sq. centimeter = 1.14 atmos.),* and we should expand to that pressure.

For condensing engines and for non-condensing engines under very high pressures, the limit is fixed by the extent of the losses of heat just described. For the common unjacketed engine at moderate speed the writer has been accustomed to assume for the rate of expansion giving maximum economy in the common single cylindered unjacketed engine, working at fair speed, $r_e < \frac{1}{2}\sqrt{P}$; for high-speed engines of best proportions and for compound engines, $r_e < \frac{3}{4}\sqrt{P}$ and $r_e < \sqrt{P}$ respectively. Where more variable conditions must be considered, he has written, for unjacketed cylinders or for ineffectively jacketed engines,

$$r^e = \frac{1}{p^v} = \frac{1}{p^c} = 0.02\sqrt{VNP_1} = 0.13\sqrt{V_m N P_m}, \text{ nearly;}\dagger$$

$$p_t = p_v = p^c = \frac{50}{\sqrt{VNP_1}} = \frac{8}{\sqrt{V_m N P_m}}, \text{ nearly,}$$

* With large ports and dry exhaust steam this figure should be reduced 10 per cent. nearly.

† Emery has proposed, as fair values, $r^a = \frac{P+37}{22}$, where P is the *gauge* pressure, and considers the values thus obtained as large for ordinary single cylinder engines, and small for the best compound engines.

where S is the speed of piston, N the number of cylinders when the engine is "compound," P_1 and P_m are the initial pressures per square inch and per square centimeter, and p_t is the terminal pressure.

For well-jacketed engines we may take, roughly,

$$r_e = \frac{1}{p_r} = \frac{1}{p_c} = 0.5 \sqrt{N P_1} = 1.75 \sqrt{N P_m}, \text{ nearly.}$$

$$p_t = p_r = p_e = \frac{2}{\sqrt{N P_1}} = \frac{0.6}{\sqrt{N P_m}}, \text{ nearly.}$$

In determining the values of the index n in the assumed expression pv^n = constant, for the equation of the expansion line, new difficulties arise. This index is itself variable in each case as expansion progresses, and no two cases give the same mean value. Rankine takes $n = \frac{10}{9}$ for expansion in non-conducting cylinders, perhaps assuming that in those engines on which he experimented the conditions were practically such as to give this value. The writer has no doubt that the steam supplied was practically dry, but Zeuner has shown that the value of n is pretty nearly

$$n = 1.035 + \frac{x}{10},$$

where x measures the proportion of steam present or the "dryness fraction." For $x = 1$, $n = 1.135$, as tabulated, and for $n = 1.111$ as taken by Rankine, we have

$$x = 10\,(1.111 - 1.035) = 0.76,$$

showing that initial condensation must have produced 24 per cent. water, a fact which introduces an error in his estimates of heat supplied to the engine from the boiler.

In none of the values above given is the fact exhibited that the effect of the phenomenon here studied at some length is to cause a rapid fall of the expansion line at the start and a considerable rise at the end, thus causing the line to depart from the curve represented by $pv^n = C$ to an extent that cannot be definitely stated. The value of the index is rendered by this cause, also, not only very variable for different cases, but it is probably usually varied constantly along the expansion line in each case by these new variable conditions. So great are these departures from any laws yet expressed by formulas that we may be justified often in tak-

ing advantage of the fact that the curve as often approaches the equilateral hyperbola as any other regular curve.

Where steam-jacketing is so efficient as to prevent condensation during expansion, and where, assuming it possible, superheating can be made so effective as to prevent initial condensation, the transfer of heat from steam to exhaust without transformation into work, in the manner here considered, would be greatly reduced, and perhaps so far as to make the limit $p_v = p_b + p_f$, as in non-condensing engines with low steam.

The compound engine, with receiver, offers peculiar opportunities to secure these conditions by superheating between the two cylinders, as has been done by Corliss, Cowper, Leavitt, and others.

The last column of the large table exhibits this case, assuming $p_v = p_t = p_b + p_f = 7\frac{1}{2}$ pounds. The value of r is increased for a given value of p_t, often to a considerable extent in actual engines, by re-evaporation.

The tabulated values of r_e may be taken as maxima for low pressures and minima for high steam, and in the latter case considerable departures from them in the direction of larger values have often produced but little difference of efficiency.

The following are values of n for various cases commonly found in real work, or taken in theoretical discussions:

VALUES OF n IN pv^n = CONSTANT.

Air, isothermal expansion,	1.0
" adiabatic "	1.4
" wet and adiabatic,	1.2
Gases generally, isothermal,	1.0
" " adiabatic,	1.4
" in explosive gas engines,	1.6
Steam, dry and saturated,	1.046
" adiabatic,	1.135
" 0.76; water, 0.24,	1.111
" superheated,	1.333
" and water generally,	$n = 1.035 + \frac{x}{10}$

The absolute values of the weights of steam used in engines under the conditions above considered cannot be predicated with any greater accuracy than the proper rates of expansion. The expenditure of heat in this method of waste increases in some undetermined ratio with the increase of the rate of expansion, and the

writer has usually anticipated a loss at least proportional to the square root of that rate, and would add a percentage equal usually to at least $h_c = 0.1 \sqrt{r_e}$, and often to $h_c = .25 \sqrt{r_e}$, to the amount calculated ordinarily by Rankine's methods, and would expect the weight of steam used to reach $W = \frac{200}{\sqrt{P_1}}$; $W = \frac{24}{\sqrt{P_m}}$; nearly, in general practice and with good engines.

Probable Terminal Pressures and Rates of Expansion at Maximum Efficiency.

Initial pressures. Absolute.		Speed of piston.		SINGLE CYLINDERS.												COMPOUND ($N = 2$) CONDENSING.								
				Non-condensing.						Condensing.														
				Unjacketed.			Jacketed.			Unjacketed.			Jacketed.			Sides jacketed.			Heads and sides jacketed.			Heads and sides jacketed with efficient superheating.		
				$p_t = p_v$		r_e	$p_t = p_v$		r_e	$p_t = p_v = p_e$		r_e	$p_t = p_v = p_c$		r_e	$p_t = p_v = p_c$		r_e	$p_t = p_v = p_c$		r_e	$p_t = p_v = p_b + p$		r_e
P_t	P_m	V	1m	Lbs. on sq in.	Kilos on sq. cm.		Lbs. on sq. in.	Kilos on sq. cm.		Lbs. on sq. in.	Kilos on sq. cm.		Lbs. on sq. in.	Kilos on sq. cm.		Lbs. on sq. in.	Kilos on sq. cm.		Lbs. on sq. in.	Kilos on sq. cm.		Lbs. on sq. in.	Kilos on sq. cm.	
40	2.8	400	122	20	1.4	2.0	20	1.4	2.0	16	1.1	2.5	13	.9	3.0	9	.6	4.5	7.5	.5	5	7½	.5	5
		625	185	20	1.4	2.0	20	1.4	2.0	13	.9	3.0	13	.9	3.0	9	.6	4.5	7.5	.5	5	7½	.5	5
60	4.2	400	122	20	1.4	3.0	20	1.4	3.0	20	1.4	3.0	15	1.1	4.0	11	.8	5.5	8	.6	7.5	7½	.5	8
		625	185	20	1.4	3.0	20	1.4	3.0	15	1.1	4.0	15	1.1	4.0	11	.8	5.5	8	.6	7.5	7½	.5	8
80	5.6	400	122	23	1.6	3.5	20	1.4	4.0	23	1.6	3.5	18	1.3	4.5	13	.9	6.5	9	.6	9	7½	.5	11
		625	185	20	1.4	4.0	20	1.4	4.0	18	1.3	4.5	18	1.3	4.5	13	.9	6.5	9	.6	9	7½	.5	11
100	7.0	400	122	25	1.8	4.0	20	1.4	5.0	25	1.8	4	20	1.4	5.0	14	1.1	7.0	10	.7	10	7½	.5	13
		625	185	20	1.4	5.0	20	1.4	5.0	20	1.4	5	20	1.4	5.0	14	1.1	7.0	10	.7	10	7½	.5	13
120	8.4	400	122	27	1.9	4.5	22	1.5	5.5	27	1.9	4.5	22	1.5	5.5	16	1.1	7.5	11	.8	10.5	7½	.5	17
		625	185	22	1.5	5.5	22	1.5	5.5	22	1.5	5.5	22	1.8	5.5	16	1.1	7.5	11	.8	10.5	7½	.5	17
150	10.5	400	122	30	2.1	5.0	25	1.8	6	30	2.1	5.0	25	1.8	6.0	18	1.3	8.5	12.5	.9	12	7½	.5	20
		625	185	25	1.8	6.0	25	1.8	6	25	1.8	6.0	25	1.1	6.0	18	1.3	8.5	12.5	.9	12	7½	.5	20
200	14.1	400	122	36	2.6	5.5	29	2.1	7	36	2.5	5.5	29	2.1	7.0	20	1.4	10.0	14	1.0	14	7½	.5	27
		625	185	29	2.1	7.0	29	2.1	7	29	2.1	7.0	29	2.1	7.0	20	1.4	10.0	14	1.0	14	7½	.5	27

$p_t = p_v$ is taken as equal to p_c except where $p_v = p_b + p_f$ exceeds p_c, when this greater value is adopted; *e.g.*, in non-condensing engines with low steam, and in highly superheated steam-engines.

Deduct 14.7 lbs. per sq. in. = 1 kg. per sq. cm. to obtain gauge pressures. Hyperbolic expansion is assumed.

In the general expression for loss by initial condensation, $h_c = a\sqrt{r_e}$, the writer has used the following values for the coefficient a:

Unjacketed cylinders, ...	$\begin{cases} 1600 \div Vd \\ 1200 \div V_m d_m \end{cases}$
Cylinders with sides jacketed, ...	0.10
Cylinders with sides and head jacketed, . .	0.07

in which d and d_m are diameters of cylinders in inches and centimeters and V and V_m are velocities of piston in feet and meters.

Similarly rough statements of probable weight of steam per horse-power and per hour for a wider range of *average* good conditions, as used by the writer, have been (minima):

$$W = \frac{40}{\log. P_1}; \quad W_m = \frac{7.7}{\log. P_m}, \text{ for fast or jacketed engines;}$$

$$W = \frac{100}{\log. (P_1 V)}; \quad W_m = \frac{20}{\log. (P_m V_m)}, \text{ for unjacketed cylinders.}$$

Where the ratios of expansion have been made those of maximum efficiency, the closest approximation has been attained by taking

$$W = \frac{15}{\log. r_e}(1 + .1\sqrt{r_e}); \quad W_m = \frac{7.}{\log. r_e}(1 + .1\sqrt{r_e}),$$

as the expenditure will in such cases approximate most nearly to a direct ratio with the net energy obtained per diagram. These values are adopted in the following table. It is evident that the higher the value of r_e, the better the type of engine, and that we are here given a good gauge by which to make comparisons of the efficiency of different kinds of engines.

These values accord moderately well with the observation and experience of the writer where engines of good design have been compared, and may possibly prove useful to others in designing or in drawing up specifications. Like the values of p_v or of r, they can only be taken as probable means, and adopted provisionally, until better and more accurate values have been determined for a wider range of conditions.

Thus, we have the following probable values of weight of steam demanded *where the ratio of expansion is correctly adjusted:*

Probable Minimum Weights of Steam per Hour per Horse-power.

r^e	W Pounds.	W_m Kilos.	r^e	W Pounds.	W_m Kilos.	r^e	W Pounds.	W_m Kilos.
3	32	15	8	20	9	13	17	8
4	27	12	9	19	9	14	16	7
5	25	11	10	19	9	16	16	7
6	22	11	11	18	9	20	15	7
7	20	9	12	17	8	25	15	7

Taking the probable minimum expenditure of coal per hour and per horse-power at *one-ninth* the weight of steam demanded, we get

$$W' = \frac{1.7}{\log. r^e}(1 + 0.1 \sqrt{r^e}),\ W'_m = \frac{.8}{\log. r^e}(1 + 0.1 \sqrt{r^e}).$$

And thus, assuming, as before, the best probable conditions and the ratio of expansion giving a minimum cost of steam, we obtain the following:

Probable Minimum Weights of Coal per Horse-power per Hour.

r^e	W'' Pounds.	W''_m Kilos.	r^e	W'' Pounds.	W''_m Kilos.	r^e	W'' Pounds.	W''_m Kilos.
3	3.5	1.6	8	2.2	1.0	13	1.9	0.9
4	3.0	1.4	9	2.1	1.0	14	1.8	0.8
5	2.8	1.3	10	2.1	1.0	16	1.8	0.8
6	2.3	1.1	11	2.0	0.9	20	1.7	0.8
7	2.2	1.0	12	1.9	0.9	25	1.7	0.8

For cases in which the boiler gives an evaporation of ten pounds of water per pound of coal we may get ten per cent. better figures.*

So far as the experience of the writer and comparisons made by him with data given by the best experiments have extended, these figures have proven so far accordant that he does not hesitate to use them in estimating probable results. Adding, say, 20 per cent. will give figures on which to base a guarantee in making up contracts for skilfully designed engines.

* A private letter, lying on the table of the writer, giving results corresponding with the case assumed as giving $r^e = 20$, states the coal consumption at 1.5 pounds. This is obtained by one of the oldest and most distinguished engineers in the United States. The boiler has about this maximum efficiency.

The introduction of this element practically completes the theory of the steam-engine. Every practising engineer will look with interest for experimentally derived data and exact expressions that may replace approximate formulas which as here provisionally used are purely empirical and have no scientific value.

How far the high efficiencies here seen to be probably attainable are worth paying for is a commercial question of great importance, but is quite distinct from that here considered.

REFERENCES.—The quantities and the empirical formulas and rules given by the writer as deduced from experience and observation may be compared with the following, which comprise nearly all that he has been able to find bearing upon the subject with any degree of definiteness: D. K. Clark, *Manual for Mechanical Engineers*, pp. 888, 890; *Northcott on the Steam-engine*, pp. 157, 158; Isherwood's *Engineering Researches; Cotterill on the Steam-engine*, chap. xi., especially pp. 294 to 296; R. H. Buel's *Addenda to Du Bois's Weisbach*, vol. ii., § 512; Rankine's *Papers;* Rankine's *Steam-engine*, § 282, 289; *Porter on the Richards Indicator*, London, 174, sect. iii.

The first table given here, which I passed over in the reading, is one that indicates what are probably the best terminal pressures and rates of expansion at maximum efficiency; and with unjacketed engines I have taken it as affected by the speed of the engine. In case of a condensing, single cylinder, unjacketed engine, 25 pounds by gauge, an expansion of 2.5 would probably be the maximum. Taking a common case, we are carrying now 80 pounds absolute, 65 pounds by gauge, for a single cylinder, condensing, unjacketed engine, and an expansion of 3.5 is indicated as being the best at 400 feet per minute, and expanding 4.5 times is about a maximum for 625 feet per minute. Carrying 100 pounds by gauge, a single cylinder, condensing, unjacketed engine would expand 4.5 times if maintained at 400 feet per minute; 5.5 times if running 625 feet per minute. Another case, an engine 80 pounds absolute, 65 pounds by gauge, would expand down to a terminal pressure of 20—that is, four expansions; 100 pounds of steam absolute, 85 pounds by gauge, would enable us to expand five times; if we can run to 150 pounds absolute, 135 by gauge, we can expand six times. Now turning to the compound engines —here is a case of a compound engine with the sides jacketed, and carrying, we will say, 80 pounds absolute, 65 by gauge; it would appear, if the expansions I have taken are right, that at

either speed, and 65 pounds by gauge, you could expand with economy 6.5 times; a compound engine, sides jacketed, with two cylinders, carrying 100 pounds by gauge, should expand 7.5 times. If we can obtain efficient superheating so as to expand to the limit produced by back pressure and friction, we would in case of compound engine, with efficient jacketing and superheating, carrying 65 pounds by gauge, expand 11 times; carrying 100 pounds by gauge, we could expand about 17 times. But that could only occur when this initial condensation was so perfectly checked as to allow us to go down to the other limits, to expand until we reach the limit set by back pressure and friction combined. If any gentlemen present have in mind any case of special economy in any engine, I would like very much to have him compare it with the figures put down there; but I would like to have it understood that, as I have stated in my paper, these quantities are simply derived from observation and are entirely empirical. There are gentlemen here who have had vastly more experience than I in each of the cases, and I would like very much to have our experiences compared.

XXXIV.

MOST ECONOMICAL POINT OF CUT-OFF IN STEAM ENGINES.

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I.

It is needless at the present time to dilate upon the importance of determining, in engines, the ratio of expansion of steam which will give the maximum efficiency for a given expenditure of money. This is attested as well by the amount of attention the question has received from engineers of high professional standing, as by the great loss of money which the adoption of some other less economical ratio of expansion implies when the immense number of steam-engines in use is considered.

In spite of the importance of the problem, the only correct solu-

* We are indebted to Mr. W. H. Weightman for valuable co-operation in the preparation of this paper.

tion which has ever been presented has received practically no attention. Before, however, putting forward this correct method, emanating from no less an authority than the great Rankine, whose well-known clearness of apprehension it exemplifies to no mean degree, we consider it well to briefly review some of the phases which the problem has assumed in this country during the past few years, though most of the later discussions and publications have had a tendency rather to retard than to advance the settlement of the question.

In 1874 the well-known trials of the engines of the United States Revenue Steamers were conducted by a Board of Naval Engineers, under the direction of Chief Engineer Loring, U. S. N., and Consulting Engineer Charles E. Emery, U. S. R. M.

In summarizing the results of these experiments, Mr. Emery attempted to deduce a rule for "the most economical point of cut-off for the pressure employed," and presented as a provisional rule $r = \frac{(P + 37)}{22}$. In regard to this formula, he says: "It is probable that these ratios are nearly correct for single engines of large size with details of good design, too large for single engines of ordinary construction, and too small for the better class of compound engines." While Mr. Emery does not claim for this rule anything but an approximate determination of the most economical point of cut-off, it is well to point out that even this claim cannot be adequately substantiated. This formula was deduced from the best results of certain careful experiments on different types of steam engines under varying conditions of expansion. There is, however, not only a lack of evidence to prove that other ratios of expansion not tried in the experiments would not have given better results in every particular case, but as a matter of fact the results presented in tabular form upon which the formula has apparently been based (see *Journal of the Franklin Institute*, February to May, 1875) show discrepancies which themselves tend to discredit the correctness of the formula. In one case a given ratio of expansion, which according to the formula should exhibit the most favorable result for the given initial pressure of steam, shows a greater water consumption than for a considerably higher ratio of expansion, while the entire argument is based upon such small and variable differences of water consumption, as to leave room for considerable conjecture as to whether they did not fall within the limits of error of such experimental determinations. In present-

ing the true method of determining the most economical point of cut-off, we select as one of the illustrations the steam machinery of the United States Revenue Steamer "Rush," one of the vessels experimented upon by the Board of Naval Engineers. We find that the most efficient ratio of expansion for a given expenditure of money varies under certain conditions, which are explained in detail, from $R = 6.35$ to $R = 3.84$, *for the same initial pressure of steam*, the controlling element in bringing about this difference arising from the consideration as to whether coal could be obtained at $10 a ton, as per Pacific Coast rates, or at $5 a ton, as per Atlantic Coast rates. We thus definitely prove not only the fallacy of Mr. Emery's formula, but also refute its practicability as a safe provisional rule.

In June, 1880,* Professor Marks, evidently believing that the best ratio of expansion would be ascertained by finding that ratio which will give the greatest efficiency of the engine for the least amount of fuel consumed, determined as the best ratio of expansion that denoted by the expression $R = \frac{P_1}{B}$, in which P_1 represents the absolute initial pressure of steam, and B the absolute back pressure of steam in cylinder. This result is equivalent to the statement that the most economical ratio of expansion, accepting the hyperbolic law, is that which will make the terminal pressure of expansion equal to the back pressure. For in the expression $P_1 V_1 = P_0 V_0$, in which P_1 = initial absolute pressure of steam, V_1 = volume of steam in cylinder at pressure P_1 before expansion, P_0 = absolute terminal pressure at end of stroke, and V_0 = volume of cylinder, we have $V_0 = R\ V_1$, and if we substitute for R, Professor Marks' result, viz.: $R = \frac{P_1}{B}$, we obtain $P_1 V_1 = P_0 \times \frac{P_1}{B} V_1$, or $P_0 = B$. It is not evident from Professor Marks' text that he recognized that his result was capable of this physical interpretation, though we feel confident that he was conscious of it.

But the determination of the ratio of expansion for maximum economy as regards the efficiency of the fluid alone, as given by Professor Marks is not theoretically correct.

No allowance is made for condensation of steam, which affects the truth of the result to an appreciable extent, and Professor Marks' measure of work performed is only correct for isothermal expansion, while his measure of cost of steam is only correct for

* See *Journal of the Franklin Institute.*

adiabatic expansion. For isothermal expansion the expenditure of heat is greater than that allowed by Professor Marks by the value in heat units of the area under the expansion line (see Appendix 1), in which case the result attained by him, $R = \frac{P_1}{B}$, no longer holds true (see Appendix 2). For adiabatic expansion a similar mode of procedure to that followed by Professor Marks gives for the best ratio of expansion $s = \frac{p_1^{1/\gamma}}{p_2^{1/\gamma}}$ (see Appendix 3) The latter result is the algebraic expression of the fact that for adiabatic expansion the best ratio is that which makes the terminal pressure equal to the back pressure, a truth which was pointed out by Rankine in his *Memoir of John Elder* in 1871. It is to be borne in mind, however, that all this refers to the efficiency of the fluid alone. However, the error pointed out above, and even the omission of not allowing for condensation of steam, would not of themselves account for the peculiar results obtained when Professor Marks' formula is put to a practical test. As we shall see later on, the determination of the true economical point of cut-off involves some important and controlling considerations which the formula neglects entirely. When the formula referred to was put to a practical test, the surprising fact was revealed that for an initial absolute pressure of steam of 100 pounds, and an absolute back pressure of 2 pounds in a condensing engine, the ratio of expansion recommended would be 50, while on the other hand, in the same engine, the mere increase of 2 pounds in the back pressure would cause the ratio of expansion recommended as best to change to 25. Engineers and manufacturers of steam-engines must have been led to believe that if this indeed was the result of theoretical investigation, the solution of the question was beyond the domain of theory. Our demonstration will show that the latter conclusion is not a fact. True theory is but the connected system of laws or principles which underlie practice, and physical phenomena conforming to law should never be hastily classed as beyond the grasp of generalization, though equal care should be taken to avoid hasty and unfounded generalization. That alone is theory in the true sense of the term, of which all logical deductions accord, not alone with the observed special phenomena under consideration, but harmonize as well with all known facts of the universe. However, the

logical deductions of Professor Marks' formula were so much in contradistinction to some special facts acquired by experience, that Professor R. H. Thurston appeared in the controversy in November, 1880,* claiming that the question could not be solved by theory. He gave approximate empirical formulæ for different types of engines, $\frac{1}{2}\sqrt{P}$, $\frac{3}{4}\sqrt{P}$, $\sqrt{P}$; and added the weight of his professional standing and of his position to the view that the problem of determining the most economical point of cut-off in steam-engines could only be ascertained by practical test in each separate engine. Essentially, Professor Thurston's formula accords within ordinary limits of pressure with that given by Mr. Emery, and similar objections apply.

But the radical defects in all of the above modes of solving the question will become evident when we now come to consider the true method.

In the *Philosophical Magazine* of 1854, the great master, Professor Rankine, says: "By increasing the ratio of expansion in a Cornish engine, the quantity of steam required to perform a given duty is diminished; and the cost of fuel and of the boilers is lowered. But at the same time, as the cylinders and every part of the engine must be made larger to admit of a greater expansion, the cost of the engine is increased. It thus becomes a problem of maxima and minima to determine what ratio of expansion ought to be adopted under given circumstances, in order that the sum of the annual cost of fuel, and the interest of the capital employed in construction, may be the least possible as compared with the work done."

This marks the keynote of our inquiry. What is here stated as a fact relative to the Cornish engine necessarily applies to any type of steam-engine, and in 1866, in a communication to the British Institution of Naval Architects, Professor Rankine outlined a simple graphical method of obtaining the most economical ratios of expansion in steam-engines based on the above principle. Judging from the discussion that followed by such men as Scott Russell, Mr. Cowper, and Mr. Napier, the paper does not seem to have been at all understood, nor its importance appreciated. This we attribute to the fact that no examples were given of the mode of procedure in particular cases, and that the method outlined, while clear, concise, and correct, was expressed in such general terms, that it lacked the "home thrust" which

* See *Scientific American*, November 20, 1880.

detail of general method and practical illustration alone can give. In the *Philosophical Magazine*, however, the mathematics of the problem is presented with an illustration of their application to the Cornish engine. That this general method should have been overlooked and forgotten, we attribute to the heading under which it was set forth, "Oeconomy of Single-acting Expansive Engines," which no longer appeals to the sympathy of the investigator of to-day.

Having at once become thoroughly impressed with the correctness of the principle enunciated by Professor Rankine, we have felt that a presentation of the same, with appropriate comments, adaptation for present practice and practical illustrations, would be of interest to builders and users of steam-engines. We have felt, too, that it was within the peculiar province of those who manufacture and are in charge of steam-engines to supply some needful data to make perfect accuracy assured, and to give the method that opportunity for application in design and daily work which will secure its greatest usefulness. We recognize that empirical formulæ, while often useful, in the main tend to delay research, and we have thought it wise to dwell upon a correct solution, even if it should teach us that we are not at the end of investigation. As a matter of fact, we are pleased to believe that the method will be found of immediate practical use, and that its use will give a satisfaction, carry with it a conviction, and assure an accuracy, which empirical formulæ heretofore employed have failed to do. It will mainly depend upon your investigations and cumulative labors, gentlemen, to fix the time when the best ratio of expansion in steam-engines will be settled beyond the chance of a doubt.

As we have seen, the problem of the best ratio of expansion is not one of economy of consumption of fuel and economy of cost of boiler alone. The question of interest on cost of engine, depreciation of value of engine, repairs of engine, etc., enters as well; for as we increase the ratio of expansion, and thus, within certain limits, fixed by the back pressure and condensation of steam, decrease the amount of fuel required, and cost of boiler per unit of work, we have to increase the dimensions of the cylinder, and the size of the engine to attain the required power. We thus increase the cost of the engine, etc., as we increase the ratio of expansion, while at the same time we decrease the fuel consumption, the cost of boiler, etc. So that there is in every engine some point of

cut-off, determinable by calculation and graphical construction, which will secure the greatest efficiency for a given expenditure of money, taking into consideration the cost of fuel, wages of firemen, interest on cost of boiler, depreciation of value of boiler, repairs to boiler, interest on cost of engine, wages of engineer, depreciation of value of engine, insurance, repairs of engine, and oil, waste, etc., used for engine. In case of freight-carrying vessels, the value of the room occupied by fuel should be considered in estimating the cost of fuel.

We will premise at once that it will be found that the cost of fuel and oil, the interest on the cost of engine and the wages of the engineer force prove the controlling elements in the determination of the best ratio of expansion, and that slight differences in either of the quantities entering the problem do not appreciably affect the result.

GRAPHICAL METHOD—GENERAL ANALYSIS.

In the accompanying diagram let the capacity of the cylinder at full stroke be represented by a horizontal base line or abscissa, drawn through the point 0, and measured by the distance from the point 0 to the point 16.44 to the right of 0. (For the sake of keeping the diagram within convenient limits, a part only of this line extending to 6.70 is shown.) Divide the base-line 0—16.44 into any number of equal parts, say 164, as indicated in the diagram. Then will 6.70 represent the volume of steam used in the cylinder per stroke when the ratio of expansion of the steam is $\frac{16.44}{6.70} = 2.45$; 3.60 represent the volume of steam used in cylinder per stroke, when the ratio of expansion is $\frac{16.44}{3.60} = 4.6$; 2.50 represent the volume of steam used in cylinder per stroke when the ratio of expansion is $\frac{16.44}{2.50} = 6.6$, etc. From the 164 equal divisions of the line 0—16.44, draw perpendiculars or ordinates to the line. Lay off on the ordinate from the point 16.44 a distance 1.000, representing the absolute initial pressure of steam in cylinder. Then find the mean absolute pressure of steam in cylinder corresponding to the different ratios of expansion, on the same scale on which the initial absolute pressure of steam, or absolute mean pressure for full stroke, is represented by 1.000. Thus assuming the steam in the cylinder to expand according to the adiabatic law, $P\ V^{10/9} = P_1\ V_1^{10/9}$, we find for the point 5.30 (corresponding to a

ratio of expansion of $\frac{16.44}{5.30} = 3.1$) the mean absolute pressure .666, and for the point 4.70 (corresponding to a ratio of expansion of $\frac{16.44}{4.70} = 3.50$) the mean absolute pressure .516, etc. Connecting the points thus found for the several ordinates, we obtain a curve of mean absolute pressures for different ratios of expansion. This curve is shown by $A\ B$ when steam is assumed to expand in cylinder, according to the adiabatic law, $P\ V^{\frac{10}{9}} = P_1 V_1^{\frac{10}{9}}$, and by $C\ D$ when steam is assumed to expand in cylinder according to Marriotte's law, $P\ V = P_1\ V_1$. In the present general analysis, for the purposes of explanation, we will consider the steam cylinder to expand according to the adiabatic law. We now lay off on a perpendicular from the horizontal zero line of the diagram the absolute back pressure of steam in cylinder on the same scale on which the initial absolute pressure of steam is represented by 1.000. Thus representing an initial absolute pressure of 85 pounds per square inch in a condensing engine by 1.000 on the diagram, a back pressure of 1.7 pounds would be represented by the line $\frac{1.7 \times 1.000}{85} = .020$. The length of the ordinates of the curve $A\ B$ from the line .020 will represent the mean effective pressure of steam in cylinder for different ratios of expansion for the particular case considered, taking the back pressure of steam alone into account. If we now assume the friction of the engine equal to a back pressure of 1.7 pounds per square inch, the total resistance of the engine itself to the forward stroke will be represented by $1.7 + 1.7 = 3.4$, which is indicated by the line $\frac{3.4 \times 1.000}{85} = .040$ on the diagram. The length of the ordinates of the curve $A\ B$ from the line 0.40 will represent the mean effective pressure of steam in cylinder for different ratios of expansion, for the particular case, taking the back pressure of steam and friction of engine into consideration.

We now proceed to find the cost of running the engine for any stated time inasmuch as that cost depends upon the capacity of the cylinder. This consists of interest on cost of engine, depreciation of value of engine, repairs to engine, oil, waste, etc., used for engine, and wages of engineer. We find also the cost of supplying the cylinder with full steam of given initial pressure. The latter consists of interest on cost of boiler, depreciation of value of boiler, repairs of boiler, wages of fireman, and cost of fuel. Following Rankine, we designate the first item *cost of engine*, and the latter

cost of full steam. These costs are expressed in terms of the same unit; for instance, in terms of dollars per hour. The exact manner in which these items are obtained will become more manifest when we come to consider special cases. Let the cost of full steam be represented by 16.44 on our diagram, then will 6 70, to the right of 0, represent the cost of steam when the ratio of expansion is $\frac{16.44}{6.70} = 2.45$, and 4.40 represents the cost of steam in the same engine when the ratio of expansion is $\frac{16.44}{4.40} = 3.74$. Lay off the cost of engine to the left of the point 0 on the same scale on which 16.44 represents the cost of full steam. Let this in the particular problem be represented by 1.73, then the total cost of running the engine at full stroke will be represented by $1.73 + 16.44 = 18.17$, while the total cost of running the engine at a ratio of expansion of 2.49 (point 6.60 on our diagram) will be $1.73 + 6.60 = 8.33$, the sum of the cost of engine, and the cost of steam for a ratio of expansion of 2.49 (volume 6.60). Recurring now to the hypothesis of neglecting back pressure and friction of engine, we draw from the point 1.73, on the horizontal zero or base line, a tangent to the curve $A\ B$, and the point at which that tangent meets the same will determine the best ratio of expansion for the particular case considered. In this case it will be at the point 3.4, corresponding to a ratio of expansion of $\frac{16.44}{3.4} = 4.84$, as shown on top of diagram, which gives a scale of ratios of expansion equivalent to scale of volume of steam in cylinder before expansion on bottom of diagram. For at this point, 3.4 (ratio of expansion 4.84), the indicated work is proportional to the length of the ordinate to the horizontal zero line, while the cost of the work is proportional to the foot of that ordinate, to the point 1.73 to the left of the vertical zero line. Therefore the work is greatest as compared to the cost, and the cost least as compared to the work, or, in other words, we attain the greatest efficiency of the engine for the least expenditure of money, when the slope of the line drawn from the point 1.73 is greatest, or when it is tangent to the curve. If we now take back pressure into consideration, as we must in every case, the tangent must be drawn from the back pressure line, giving a different ratio of expansion than in the hypothetical case just considered. For the work is represented for different ratios of expansion by the distance of the curve $A\ B$ from the back pressure line, and not, as in the hypothetical case, from the zero line. Similarly we proceed

when we include friction of engine as well as back pressure of steam, drawing the tangent to the curve from a point a distance representing the cost of engine, to the left of the zero ordinate, and on the line representing back pressure of steam plus friction of engine.

This defines the general method of procedure for determining in steam-engines the ratio of expansion which will give the maximum efficiency for the least expenditure of money. As was said at the outset, it is based upon and incorporates that given by Rankine, and while we have attempted by introducing some hypothetical details to convey a more manifest idea in a short time than that which Rankine's analysis, clear and lucid as it actually is, conveys, we feel that the true significance and ready applicability of the method will not be fully appreciated until we give a practical exposition of the method in detail in considering special cases.

Before, however, devoting our attention to this interesting phase of our problem, we are obliged to give some consideration to the method of determining the curve of mean absolute pressures for different ratios of expansion. We find that for the purposes of this investigation the discrepancies between the expansion lines of actual indicator cards and the $^{10}|_9$ adiabatic curve are comparatively small, and therefore the $^{10}|_9$ adiabatic has been used in this investigation as best fulfilling the theoretical qualifications of the analysis. We have, however, constructed the hyperbolic curve of mean pressures as well and given a few examples of its use to illustrate to what extent the discrepancies above referred to are effective in the results of the method. We append tables of mean pressures for given ratios of expansion for hyperbolic and adiabatic expansion of steam respectively:

EXPANSION OF STEAM ACCORDING TO MARRIOTTE'S LAW—
$(P\ V = P_1\ V_1)$.

[Calculated from formula $R = \frac{1}{r}(1 + \log_e r)$ in which $R =$ ratio of absolute initial pressure (p_1) of steam in cylinder to mean absolute pressure (p) of steam in cylinder, or $= \frac{p}{p_1}$ and $r =$ ratio of initial volume (v_1) of steam in cylinder at pressure p_1 to final volume (v_2) of steam at final pressure p_2, or $= \frac{v_2}{v_1}$.]

Ratio of expansion.	Ratio of initial to mean pressure.	Ratio of expansion.	Ratio of initial to mean pressure.	Ratio of expansion.	Ratio of initial to mean pressure.
r.	*R.*	*r.*	*R.*	*r.*	*R.*
1.00	1.000	3.50	.644	5.75	.478
1.25	.978	3.60	.634	6.00	.465
1.50	.937	3.70	.624	6.25	.453
1.75	.891	3.80	.614	6.50	.442
2.00	.847	3.90	.605	6.75	.431
2.10	.829	4.00	.597	7.00	.421
2.20	.813	4.10	.588	7.25	.411
2.30	.797	4.20	.580	7.50	.402
2.40	.781	4.30	.572	7.75	.393
2.50	.766	4.40	.564	8.00	.385
2.60	.752	4.50	.556	8.25	.377
2.70	.738	4.60	.549	8.50	.369
2.80	.725	4.70	.542	8.75	.362
2.90	.712	4.80	.535	9.00	.355
3.00	.700	4.90	.528	9.25	.349
3.10	.688	5.00	.522	9.50	.342
3.20	.676	5.25	.506	9.75	.336
3.30	.665	5.50	.492	10.00	.330
3.40	.654	......	......	......	......

ADIABATIC EXPANSION OF STEAM—($PV^{\frac{10}{9}} = P_1 V_1^{\frac{10}{9}}$).

[Calculated from formula $R = 10\, r^{-1} - 9\, r^{-\frac{10}{9}}$ in which $R =$ ratio of initial absolute pressure (p_1) of steam in cylinder to mean absolute pressure (p) of steam in cylinder, or $\frac{p}{p_1}$ and $r =$ ratio of initial volume v_1 of steam in cylinder at pressure p_1 to final volume (v_2) of steam at final pressure (p_2), or $= \frac{v_2}{v_1}$.]

Ratio of expansion.	Ratio of initial to mean pressure.	Ratio of expansion.	Ratio of initial to mean pressure.	Ratio of expansion.	Ratio of initial to mean pressure.
r.	*R.*	*r.*	*R.*	*r.*	*R.*
1.00	1.000	3.50	.620	5.75	.450
1.25	.976	3.60	.610	6.00	.438
1.50	.931	3.70	.600	6.25	.425
1.75	.881	3.80	.590	6.50	.413
2.00	.834	3.90	.580	6.75	.403
2.10	.815	4.00	.571	7.00	.393
2.20	.798	4.10	.562	7.25	.383
2.30	.781	4.20	.554	7.50	.374
2.40	.765	4.30	.546	7.75	.365
2.50	.748	4.40	.538	8.00	.357
2.60	.733	4.50	.530	8.25	.349
2.70	.719	4.60	.523	8.50	.342
2.80	.704	4.70	.516	8.75	.335
2.90	.691	4.80	.509	9.00	.328
3.00	.678	4.90	.502	9.25	.321
3.10	.666	5.00	.495	9.50	.315
3.20	.654	5.25	.479	9.75	.309
3.30	.642	5.50	.464	10.00	.303
3.40	.630	......	......	......	......

GRAPHICAL METHOD—SPECIAL CASES.

I. STEAM MACHINERY OF U. S. REVENUE STEAMER "RUSH."

Case a^1.

COST OF FULL STEAM.

Cost of Coal.

We learn from the experiments of the naval engineers on the steam machinery of the "Rush" that when the initial absolute pressure of steam was 83.4 pounds per square inch and back pressure 1.73 pounds, the steam expanding 6.22 times, that water consumed per hour was 4900 pounds, equivalent to 18.38 pounds of water and 2.16 pounds of coal per indicated horse-power per hour. Accepting these data, we find the coal consumed per hour equal to $\frac{2.16 \times 4900}{18.38} = 575.75$ pounds. On the Pacific Coast, the present station of the "Rush," the price of coal will be, say, $10 per ton of 2240 pounds. This would give a cost of $\frac{575.75 \times 10}{2240} = \$2.57$ per hour for coal. The cost of coal for full steam would be $2.57 \times 6.22 =$ $15.98 per hour.

Cost of Firemen per Hour.

Wages of two firemen and two coal passers = $126 per month. This equals $\frac{126}{30 \times 24} = \$0.17$ per hour.

Rations of two firemen and two coal passers equal $36 per per month, or .05 per hour.

Interest on Cost of Boilers.

Cost of boilers $12,000, at 6 per cent., gives $0.08 interest per hour.

Depreciation of Boilers.

Take the life of a boiler as twelve years, and let it decrease $^1|_{12}$ of its original value each year, we then find depreciation of value of boilers per hour equal to $\frac{12.000}{12 \times 12 \times 30 \times 24} = \$0.116$ per hour.

Repairs to Boilers.

Let this equal say $360 per year, or $\frac{360}{12 \times 30 \times 24} = \$0.04$ per hour. Interest on this amount we will neglect.

We thus obtain total cost of full steam equal to $15.98 + .17 + .05$ $+ .08. + 12 + .04 = \$16.44$ per hour.

COST OF ENGINE.

Interest on Cost of Engine.

Price = \$28,000, at 6 per cent., gives $\frac{28,000 \times .06}{360 \times 24}$ = \$0.194 per hour.

Wages of Engineers.

Three engineers receive, jointly, salary, ...	\$4500 a year.
Two oilers " " " . . .	1080 "
Total salaries for engineer force, . . .	\$5500 per year.

which equals $\frac{5580}{360 \times 24}$ = \$0.645 per hour.

Rations of Engineer Force.

Amount, \$45 a month, or \$0.063 per hour.

Depreciation of Engines.

Take life of engines to be 25 years, and let them decrease $^1/_{25}$ of their original value each year, we thus find depreciation of value of engine per hour equal to $\frac{28,000}{25 \times 30 \times 12 \times 24}$ = \$0.13 per hour.

Repairs to Engine.

Say 2 per cent. of original cost, or \$560 per year. This equals $\frac{560}{30 \times 12 \times 24}$ = \$.065 per hour. We neglect interest.

Cost of Oil.

Four gallons per twenty-four hours at 80 cents a gallon equals \$3.20 per twenty-four hours = .133 per hour.

Cost of Waste.

Too slight to be taken into account.

We thus obtain the total cost of engine = .194 + .645 + .063 + .130 +.065 + .133 = \$1.23 per hour, as compared to a "cost of full steam" of \$16.44 per hour.

We lay off to the left of the point 0 a distance of 1.23, and this represents cost of engine on the same scale on which 16.44 to the right of the point 0 represents cost of full steam. The back pressure line will be represented on the diagram by the line $\frac{1.73 \times 1.000}{83.4}$ = .021, and thus we find the point a^1 on the diagram from which the tangent is to be drawn. When drawn to the adiabatic curve it gives a ratio of expansion 5.50 and to the Marriotte curve a ratio of expansion 6.31 as that ratio which will give the maximum efficiency for a given expenditure of money.

Case a^2.—Allowing the friction to be equal to 1.73 pounds per square inch, and accepting the other conditions of Case a^1, we find as the total back pressure to forward stroke of engine, 3.46 pounds per square inch, which will be represented on the diagram by the line $\frac{3.46 \times 1.000}{83.4} = .042$, and we obtain the point a^2 from which the tangent is drawn. When drawn to the adiabatic curve it gives a ratio of expansion 5.07, and when drawn to the Marriotte curve a ratio of expansion 5.79, as that ratio which will give the maximum efficiency for a given expenditure of money.

Case a^3.—For the purposes of further demonstration of the method, assume the conditions of Case a^1 to be in force, with the exception that, instead of a water consumption, as given above, 30 per cent. more steam would have been used. Then, analyzing the cost of full steam, we find this cost \$16.44, to consist of the cost of coal, \$15.985, and the constant cost for interest on boiler, wages, etc., \$0.45. Therefore, in our assumed case of 30 per cent. greater water consumption, the cost of coal is $15.985 \times 1.3 = 20.781$, so that the total cost of full steam will be $20.781 + 0.45 = 21.231$. The length 16.44 on our diagram to the right of the 0 ordinate will now represent 21.231, and 1.23 to the left of the zero ordinate will be represented by $\frac{1.23 \times 16.44}{21.231} = .96$. We thus obtain the point a^3, from which tangent is to be drawn, which gives as the best ratio of expansion when steam expands according to adiabatic $(\frac{10}{9})$ law the figure 6.35, and when steam expands according to the Marriotte law the figure 7.33.

Case a^4.—Accepting the conditions of Case a^1, let us assume that the "Rush" was on the Atlantic Coast instead of the Pacific, and we would have to pay \$5 per ton of coal, instead of \$10 per ton. The cost of coal in Case a^1 was represented by 15.98, and so it would now be only $\frac{15.98 \times 5}{10} = \$7.99$ per hour. Adding to this the constant cost for interest on boiler, wages, etc., 0.45, we obtain $7.99 + 0.45 = \$8.44$ as the total cost of full steam for this case. \$8.44 is represented in the diagram by the distance 16.44, and, therefore, 1.23, the cost of engine, will be represented by the distance $\frac{1.23 \times 16.44}{8.44} = 2.41$ to the left of the zero ordinate. We thus obtain, remembering that the back pressure is 1.73, the point a^4, from which a tangent drawn to the adiabatic curve fixes the best ratio of expansion at 3.94, and when drawn to the Marriotte curve fixes it at 4.29.

Case a^5.—Accepting the conditions of Case a^4, assume a water consumption 30 per cent. greater than that recorded in the experiment, and we obtain for the total cost of fuel $\frac{15.985 \times 1.3 \times 5}{10} = \$10.39$. Adding to this the constant cost for wages of firemen, etc., 0.45, we obtain \$10.84 as the total cost of full steam her hour. On the same scale in which 10.84 is represented by 16.44 on our diagram, 1.23, the cost of engine, will be represented by $\frac{1.23 \times 16.44}{10.84} = 1.87$ to the left of the zero ordinate. We thus obtain the point a^5, from which a tangent drawn to the adiabatic curve gives the most economical ratio of expansion to be employed as equal to 4.40. Using the Marriotte curve gives 4.95.

II. Porter-Allen Engine.

Cylinder 9" diameter × 10" stroke. Steam pressure by gauge, 120 pounds. 600 revolutions per minute.

Case b^1.—Back pressure of steam = 16 pounds. No allowance made for clearance, and none for condensation in cylinder.

COST OF FULL STEAM.

Cost of Coal.

Volume of cylinder in cubic inches $= 9 \times 9 \times .7854 \times 10 = 636.174$. Cubic feet of steam required in engine per hour when engine follows full stroke $= \frac{636.174 \times 2 \times 600 \times 60}{1728} = 26507.25$. Weight of steam in pounds $= 26507.25 \times .3 = 7952.175$. Assuming an evaporation of the boiler of 9 pounds of steam per pound of coal, we find the coal required per hour for full steam to be equal to $\frac{7952.175}{9} = 883.575$ pounds. Let price of coal be \$5 per ton of 2000 pounds. We find cost of coal per hour to be equal to $\frac{883.575 \times 5}{2000} = \$2.209$.

Wages of Fireman.

\$2 per day of ten hours, \$0.200 per hour.

Interest on Cost of Boiler.

\$2500 at 6 per cent. equals \$150 per year = \$.017 per hour.

Depreciation of Value of Boiler.

Assume life of boiler to be $12\frac{1}{2}$ years, and let it decrease 1-$12\frac{1}{2}$ of its original value each year, then the depreciation of value of boiler per hour equals $\frac{2500 \times 2}{25 \times 12 \times 30 \times 24} = \$.023$ per hour.

Repairs of Boiler.

Let this equal $100 a year or $.012 per hour.

We thus obtain total cost of full steam equal to 2.209 + .200 + .017 + .023 + .012 = 2.461 per hour.

COST OF ENGINE.

Interest on Cost of Engine.

Assume the actual price of engine, foundations, connections, etc., complete to be $1500. Then the interest on this at 6 per cent. equals $\frac{1500 \times 6}{100 \times 360 \times 24}$ = $.01 per hour.

Wages of Engineer.

$2.50 per day of ten hours, or $0.25 per hour.

Depreciation of Value of Engine.

Assume life of engine to be twenty years, and engine to decrease $^1|_{20}$ of its original value each year, we then find depreciation of value of engine to be $75 per year or $.009 per hour.

Repairs of Engine.

Say $36 per year, or equal to $.004 per hour.

Oil and Waste.

Say $.02 per hour.

We thus obtain the cost of engine = .01 + .25 + .009 + .004 + 02 = $.293 per hour, as compared to "cost of full steam," of $2.461 per hour.

On the same scale (that of our diagram), on which 2.461, "cost of full steam," is represented by 16.44, 0.293, the "cost of engine," will be represented by $\frac{16.44 \times 0.293}{2.461}$ = 1.957 to the left of the point 0.

The back pressure 16 pounds per square inch will be represented on our diagram by the horizontal line $\frac{16 \times 1.000}{(120 + 14.7)}$ = .119, and thus we find the point b' from which the tangent is to be drawn. When drawn to the adiabatic curve, we obtain a ratio of expansion, 3.30, and to the Marriotte curve a ratio of expansion, 3.57, as that ratio which will give the maximum efficiency for a given expenditure of money.

Case b^2.*—Retaining the conditions of Case b^1, assume a clear-

* The allowance for clearance has been accidentally taken in this case, as well as in all other cases of the Graphical Method. as proportional to the ratios of ex-

ance of 5 per cent. Then will cost of coal equal 2.209 + 1.(5 = \$2.319. Cost of full steam will equal 2.319 + (2.461 — 2.209 = .252) = 2.571. On the same scale on which 2.571, "cost of full steam," is represented by 16.44, .293, the "cost of engine," will be represented by $\frac{.293 \times 16.44}{2.571} = 1.874$ to the left of the 0 ordinate, and we thus obtain the point b^2 on the diagram, from which the tangent is to be drawn. When drawn to the adiabatic curve we obtain the best ratio of expansion to be 3.35, to the Marriotte curve, 3.65.

Case b^3.—Retain the condition of Case b^2, and assume a steam consumption of 30 per cent. greater than that in Case b^2, owing to condensation of steam in cylinder. Then the cost of coal will equal $2.319 \times 1.3 = \$3.0147$. Adding to this the constant quantity for wages of fireman, etc., 0.252, we obtain the "cost of full steam," equal to \$3.267. On the same scale in which 3.267, "cost of full steam," is represented by 16.44; .293, the "cost of engine," will be represented by $\frac{293 \times 16.44}{3.267} = 1.474$, to the left of the zero ordinate, and we thus obtain the point b^3 on the diagram, from which the tangent is to be drawn. When drawn to the adiabatic curve, we obtain as best ratio of expansion to be employed, the value 3.63, and when drawn to the Marriotte curve, 3.91.

Case b^4.—Assume that the conditions of Case b^3 remain unaltered, with the exception that the friction of the engine is estimated as equal to 1 pound back pressure. Then the total resistance to the forward stroke is represented by $16 + 1 = 17$ pounds. This will be indicated on our diagram by the line $\frac{17 \times 1.000}{(120+14.7)} = .126$, and so we determine the point b^4, from which the tangent is to be drawn.

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pansion, instead of allowing a constant quantity for clearance for all ratios of expansion. This trifling neglect produces no change of any significance in the results obtained. To show how to take clearance into account in the strictly correct manner, we will consider the conditions of case b^2. The probable best ratio of expansion will be say 3.5; so we multiply .05 by 3.5, obtaining .175. Multiply 2.209 by 1.175 and obtain 2.596 and add to this $.252 = 2.848$. On the same scale on which 2.848 is represented by 16.44, .293, the "cost of engine," will be represented by $\frac{.293 \times 16.44}{2.848} = 1.691$ to the left of the zero ordinate, and we thus obtain a point on the diagram from which the tangent drawn to the adiabatic curve fixes the best ratio of expansion at 3.46. 3.35 and 3.46 will be found to give practically the same efficiency of the engine, and thus it will be seen that the slight error introduced in the method of allowing for clearance is of no practical significance.

When drawn to the adiabatic curve, we obtain as the best ratio to be employed the figure 3.55, and when drawn to the Marriotte curve, the figure 4.01.

III. Ferry-boat Engine.

c. Absolute initial pressure of steam, . . .	= 34 7 pounds.	
Back pressure, ...	= 1.5	"
Friction of engine, ...	= 1.5	"

Cylinder, 40″ diameter by 10 stroke.
Revolutions : 26 per minute.

Cost of boilers, ...	$5,500.
" engine, ...	19,500.
Life of boiler, ...	10 years.
" engine, ...	20 years.
Cost of coal, ...	$4.58 per ton of 2000 pounds.

9 pounds of water evaporated per pound of coal.

COST OF FULL STEAM.

Cost of Coal.

$$\frac{4.58 \times 3\frac{1}{3} \times 3\frac{1}{3} \times .7854 \times 20 \times 26 \times 60 \times .08657}{2000 \times 9} = \$6.004 \text{ per hour.}$$

Clearance, 5 per cent., adds .3002 to cost of coal, or raises cost of coal to $6.3042.

Allowing for 25 per cent. greater steam consumption than given by indicator card, owing to condensation of steam in cylinder, we obtain cost of coal for full steam = $6.3042 + 1.5760 = $7.8802.

Wages of fireman, ...	= .139	per hour.
Interest on cost of boilers, ...	= .038	"
Depreciation of value of boilers, ...	= .068	"
Repairs to boiler, ...	= .027	"
Total cost of full steam, ...	= $8.152	"

COST OF ENGINE.

Interest on cost of engine, ...	= .135	per hour.
Wages of engineers, ...	= .278	"
Depreciation of engine, ...	= .113	"
Repairs to engine, ...	= .049	"
Cost of oil and waste	= .030	"
Total "cost of engine,"	= .605	"

Cost of full steam being 8.152; cost of engine will be represented on our diagram by $\frac{.605 \times 1644}{8.152} = 1.22$ to the left of the zero ordinate.

The resistance to the forward stroke of the engine is 3 pounds per square inch, which is represented on our diagram by the line

$\frac{1.000 \times 3}{34.7} = .086$. We thus obtain the point c, from which a tangent drawn to the adiabatic curve shows the best ratio of expansion to be employed to equal 4.30.

IV. Buckeye Engine.

Cylinder 26″ diameter by 48″ stroke. Steam pressure by gauge, 75.3, corresponding to absolute initial pressure of $75.3 + 14.7 = 90$ pounds. 100 revolutions per minute.

Case e^1.—Assume clearance $2\frac{1}{2}$ per cent.; a steam consumption 30 per cent. greater than that called for by indicator card, owing to condensation of steam in cylinder; back pressure, 15.7 pounds; friction, 2 pounds. Price of coal, $5 per ton of 2000 pounds. Evaporation of 9 pounds of water per pound of coal.

COST OF FULL STEAM.

Cost of Coal.

Then cost of coal per hour when engine follows full stroke, and no allowance is made for clearance and condensation, equals

$$\frac{26 \times 26 \times .7854 \times 8 \times 100 \times 60 \times 5 \times .21185}{12 \times 12 \times 2000 \times 9} = \$10.415.$$

Allowing $2\frac{1}{2}$ per cent. clearance adds $0.260 to cost of coal, or raises the amount to $10.675.

Allowing for 30 per cent. condensation of steam, we obtain cost of coal for full steam $= \$10.675 \times 1.3 = \$13.877$ per hour.

Wages of Firemen.

One fireman and one coal-passer = $3.50 a day of ten hours, or $0.350 per hour.

Interest on Cost of Boilers.

$8500 at 6 per cent. equals $0.59 per hour.

Depreciation of Value of Boilers.

Assume life of boilers to be twelve years, and let them decrease $\frac{1}{12}$ of their original value each year, then the depreciation of value of boilers per hour equals $\frac{8500}{12 \times 12 \times 30 \times 24} = .082$ per hour.

Repairs of Boilers.

Let this equal $190 a year, or .022 per hour, + interest $.001 = .023$.

We thus obtain total cost of full steam equal to 13.877 + .350 + .059 + .082 + .023 = \$14,391 per hour.

COST OF ENGINE.

Interest on Cost of Engine.

Assume the actual price of engine, foundations, connections, etc., complete to be \$9.000 Then the interest on this amount at 6 per cent. equals $\frac{9000 \times 6}{100 \times 360 \times 24} = .062$ per hour.

Wages of Engineer.

\$4 per day of ten hours, or \$0.40 per hour.

Depreciation of Value of Engine.

Assume life of engine to be twenty-five years, and engine to decrease $\frac{1}{25}$ of its original value each year, we then find depreciation of value of engine to be $\frac{9000}{25 \times 30 \times 12 \times 24} = \$.042$ per hour.

Repairs of Engine.

Say \$150 per year, or equal to .017 per hour + interest .001 gives .018 per hour.

Oil and Waste.

Say 62 cents a day, or .062 per hour.

We thus obtain the "cost of engine" = .062 + .40 + .042 + .018 + .062 = \$0.584 per hour, as compared to a cost of full steam of \$14.391 per hour.

On the same scale (that of our diagram) on which 14.391, "cost of full steam," is represented by 16.44, .584, cost of engine, will be represented by $\frac{.584 \times 16.44}{14.397} = .667$ to the left of the line 0.

The back pressure 15.7 + the friction 2 pounds = 17.7 pounds resistance to the forward stroke of the piston. This will be represented on our diagram by the horizontal line $\frac{1.000 \times 17.7}{90} = .197$, and thus we find the point e^1 from which the tangent is to be drawn to the adiabatic curve. This tangent meets the curve at the point 3.53, which represents the ratio of expansion that will give the maximum of efficiency for a given expenditure of money.

Case e^2.—Retain the conditions of *Case e^1*, with exception that engine is run as a condensing engine instead of non-condensing. Assume the back pressure in this case to be 3 pounds per square

inch. This added to friction, 2 pounds, gives a resistance of 5 pounds per square inch to the forward stroke of the piston. On the same scale on which 90 pounds pressure is represented by 1.000, 5 pounds will be represented by the horizontal line $\frac{1.000 \times 5}{90} =$ .056, and thus we find the point e^2 from which the tangent is to be drawn to the adiabatic curve. This tangent meets the curve at the point 6.28, which represents the best ratio of expansion to be employed.

Case e^3.—Retain the conditions of Case e^2, with exception that allowance is made for a steam consumption of 40 per cent. greater than that called for by the indicator card, owing to condensation of steam in cylinder, instead of 30 per cent. as in Case e^2 and Case e^1. Then cost of full steam will equal $10.675 \times 1.4 + .514 =$ \$15.459 per hour. On the same scale (that of our diagram) on which 15.459 is represented by 16.44, .584, cost of engine, will be represented by $\frac{.584 \times 16.44}{15.459} = .621$ to the left of the line 0. We thus fix the point e^3 from which the tangent is to be drawn to the adiabatic curve. This tangent meets the curve at the point 6.49, which is the most efficient ratio of expansion to be used.

V. Corliss (Pawtucket, R. I.) Pumping Engines.

Cylinders, $\left\{ \begin{array}{l} 15'' \text{ diameter} \times 30'' \text{ stroke.} \\ 30'' \text{ diameter} \times 30'' \text{ stroke.} \end{array} \right.$

Absolute initial pressure of steam = 140 pounds per square inch; 45 revolutions per minute.

Case d.—Assume clearance 1 per cent., a steam consumption 10 per cent. greater than that called for by indicator card, owing to condensation of steam in cylinder, back pressure 1.5 pounds per square inch, friction equivalent to 2 pounds square inch. Cost of coal \$5 per ton of 2000 pounds. Evaporation of 9 pounds of water per pound of coal.

COST OF FULL STEAM.

Cost of Coal.

The cost of coal per hour when engine follows full stroke and no allowance is made for clearance and condensation, equals $\left.\right\} \frac{30 \times 30 \times .7854 \times 5 \times 45 \times 60 \times 5 \times .31385}{12 \times 12 \times 2000 \times 9} = \$5.777$

Allowing 1 per cent. clearance adds \$.058 to cost of coal, or raises the amount to \$5.835.

Allowing for 10 per cent. condensation of steam, we obtain cost of coal for full steam = \$5.835 × 1.1 = \$6.418 per hour.

Wages of Fireman.

Say \$2 per day of ten hours or \$.20 per hour.

Interest on Cost of Boilers.

\$9000 at 6 per cent., equals \$.062 per hour.

Depreciation of Value of Boilers.

Assume life of boilers to be twelve years, and let them decrease $^1/_{12}$ of their original value each year, then the depreciation of value of boilers per hour equals $\frac{9000}{12 \times 12 \times 30 \times 24} = .087$ per hour.

Repairs of Boiler.

Let this equal \$240 per year, or \$0.028 per hour + interest .001 = \$.029 per hour.

We thus obtain total cost of full steam equal to 6.418 + .20 + .062 + .087 + .029 = \$6.796 per hour.

COST OF ENGINE.

Interest on Cost of Engine.

\$16,000 at 6 per cent. = \$0.111 per hour.

Wages of Engineer.

\$3.50 per day of ten hours, or \$.35 per hour.

Depreciation of Value of Engine.

Assume life of engine to be twenty-five years and engine to decrease $^1/_{25}$ of its original value each year, we then find depreciation of value of engine to be $\frac{16{,}000}{25 \times 30 \times 12 \times 24} = \$.074$ per hour.

Repairs of Engine.

Say \$230 per year, or \$.027 per hour.

Cost of Oil and Waste.

Say \$.060 per hour.

We thus obtain the "cost of engine" = 0.111 + 0.35 + .074 + .027 + .060 = \$0.622 per hour as compared to a cost of full steam of \$6.796 per hour.

On the same scale (that of our diagram) on which \$6.796, "cost

of full steam," is represented by 16.44, .622, "cost of engine," will be represented by $\frac{.622 \times 16.44}{6.796} = 1.505$.

The back pressure 1.5 + the friction 2 pounds = 3.5 pounds per square inch resistance to the forward stroke of the piston. This will be represented on our diagram by the horizontal line $\frac{1.000 \times 3.5}{140} = .025$, and thus we find the point d from which the tangent is to be drawn to the adiabatic curve. This tangent meets the curve at the point 4.85, which represents the ratio of expansion for maximum efficiency for a given expenditure of money.

Case d^1.—Retain the conditions of case d, with the exception, that, as a matter of interest to see how this will affect the result, we omit the items of interest on cost of engines, cost of repairs, and depreciation of value of engines. If we omit these items, which amount to .111 + .027 + .074 = \$0.212 per hour, we obtain "cost of engine" = .622 − .212 = \$0.410 per hour as compared to a "cost of full steam" of \$6.796 per hour.

On the same scale (that of our diagram) on which 6.796, "cost of full steam," is represented by 16.44, 0.410, "cost of engine," will be represented by $\frac{.410 \times 16.44}{6.796} = .992$. We thus find the point d^1 from which the tangent is to be drawn to the adiabatic curve. This tangent meets the curve at the point 6.09, which represents the ratio of expansion for maximum efficiency for a given expenditure of money. It is interesting to compare this result (6.09) with the result in case d (4.85). The comparison shows what an influence the wages of engineer and cost of oil, independent of the interest, repairs, and depreciation of engine, have upon the problem, when the latter items are neglected entirely.

ANALYTICAL METHOD—GENERAL ANALYSIS.

Let W = weight in pounds of steam used by engine per hour.
Let v_1 = volume of one pound of steam in cubic feet.
Let l = length of stroke in feet.
Let A = area of piston of low pressure cylinder in square feet.
Let n = number of single strokes per hour.
Let p_1 = initial absolute pressure of steam per square foot.
Let p_2 = back pressure per square foot, including back pressure of steam + friction of engine.
Let p_m = mean (absolute) pressure per square foot.
Let s = ratio of expansion (integral number).

Let K = interest per hour of cost of engine, plus cost of engineer's wages, plus cost of repairs of engine, plus depreciation of value of engine, plus cost of oil and waste—all reckoned per square foot of piston per hour.

Let h = cost of making one pound of steam per hour, including cost of fuel, interest on cost of boiler, depreciation of value of boiler, repairs of boiler, and wages of firemen.

Let $\frac{1}{v_1}$ = weight of cubic foot of steam.

Then the useful work performed by engine per hour will equal

$$p_m\, Aln - p_2\, Aln.$$

The cost of this work will be = volume of steam used $\times \frac{1}{v_1} \times h + KA$. For adiabatic expansion, volume of steam used $= \frac{Aln}{s}$, therefore cost $= \frac{hAln}{sv_1} + KA$. Let Z = "the gross mechanical action of unity of weight of steam on one side of the piston." (See paper "On the Mechanical Action of Heat," by Professor W. J. M. Rankine, *Philosophical Magazine*, 1854, page 176.)

Then $Z = \frac{p_m s}{p_1} \therefore p_m = \frac{Z}{s} p_1$ and

Useful work performed by engine per hour will equal $\frac{Z}{s} p_1\, Aln - p_2\, Aln$.

$$\text{Now } Z^* = \left[\frac{1}{1-\frac{1}{\gamma}} - \frac{\frac{1}{\gamma}}{1-\frac{1}{\gamma}}\, s^{\,1-\frac{1}{\gamma}}\right] = \left[\frac{\gamma}{\gamma-1} - \frac{1}{\gamma-1} s^{\,1-\gamma}\right]$$

(γ = exponent in the well-known formula for adiabatic expansion PV^γ = Constant.

Therefore useful work per hour equals

$$p_1 \frac{Aln}{s}\left(\frac{\gamma}{\gamma-1} - \frac{1}{\gamma-1} s^{\,1-\gamma}\right) - p_2 Aln.$$

and the efficiency of the engine for a given expenditure of money equals

$$y = \frac{p_1 \frac{Aln}{s}\left(\frac{\gamma}{\gamma-1} - \frac{1}{\gamma-1} s^{\,1-\gamma}\right) - p_2 Aln}{\frac{hAln}{sv_1} + KA}$$

To obtain a maximum value of y we have

$$\frac{dy}{ds} = \left(- p_1 \frac{Aln}{s^2}\frac{\gamma}{\gamma-1} + p_1 \frac{Aln}{s^2}\frac{s^{1-\gamma}}{\gamma-1} + s^{-\gamma}\frac{p_1 Aln}{s}\right)\left(\frac{hAln}{sv_1} + KA\right)$$
$$+ \left[p_1 \frac{Aln}{s}\left(\frac{\gamma}{\gamma-1} - \frac{1}{\gamma-1} s^{\,1-\gamma}\right) - p_2 Aln\right]\frac{hAln}{v_1 s^2} = 0$$

* See Appendix, No. 4.

Therefore

$$-\frac{p_1\, Aln\, Aln\, h}{s^3 v_1}\frac{\gamma}{\gamma-1}+\frac{p_1\, Aln\, Aln\, h}{s^3 v_1}\frac{s^{1-\gamma}}{\gamma-1}+\frac{s^{-\gamma} p_1\, Aln\, h\, Aln}{s^2 v_1}$$

$$+\frac{p_1\, Aln\, Aln\, h}{v_1 s^3}\frac{\gamma}{\gamma-1}-\frac{p_1\, Aln\, Aln\, h}{s^3 v_1}\frac{s^{1-\gamma}}{\gamma-1}-\frac{p_2\, Aln\, Aln\, h}{v_1 s^2}$$

$$+\left(p_1\frac{Aln}{s_2}\frac{s^{1-\gamma}}{\gamma-1}-\frac{p_1\, Aln}{s^2}\frac{\gamma}{\gamma-1}+\frac{s^{-\gamma}\, p_1\, Aln}{s}\right)KA=0$$

which reduces to

$$\frac{h\,Aln}{v_1}\left(p_1\,(s)^{-\gamma}-p\right)=KA\left[\frac{p_1}{\gamma-1}(\gamma-\gamma\,[s]^{\,1-\gamma})\right]\quad\text{(Equation 1).}$$

Substituting for γ its value $^{10}|_9$ for adiabatic expansion of steam in cylinder, corresponding to the formula $PV^{10}|_9=P_1V_1{}^{10}|_9$, we obtain

$$\frac{hAln}{v_1}\left(p_1\,(s)^{-10|_9}-p_2\right)=KA\left[p_1\,(10-10\,(s)^{-1|_9})\right]\quad\text{(Equation 2).}$$

In determining Equation 2, for the sake of convenience in mathematical demonstration, no allowance has been made for clearance and condensation of steam in cylinder. In actual practice there is clearance, and condensation occurs. Allowance for this condensation is made in Equation 2 by adding such a percentage for condensation to the term ($\frac{hAln}{v_1}$ + steam used for clearance), as determined by experience for the particular ratio of expansion in the particular engine considered. Thus letting i = percentage of clearance (in decimal numbers) of stroke capacity of cylinder, and c = percentage of steam (in decimal numbers) used in cylinder above that called for by the value of $\frac{Aln}{v_1}(1+i)$, which latter expression represents amount of steam used for full stroke if there is no condensation, then Equation 2 becomes

$$(1+i)(1+c)\frac{hAln}{v_1}(p_1\,(s)^{-10|_9}-p_2)=KA\left[p_1\,(10-10\,(s)^{-1|_9})\right]=\text{Equation }2A.*$$

* In this equation 2A, the allowance for clearance has been accidentally taken as proportional to the ratios of expansion, instead of allowing a constant quantity for clearance for all ratios of expansion. This trifling neglect produces no change of any significance in the results obtained, as has already been pointed out in the "Graphical Method." For refinement of accuracy, however, considering the clearance to be constant for all ratios of expansion, Equation 2A should read

$$(1+c)(1+si)\frac{hAln}{v_1}(p_1\,(s)^{-10|_9}-p_2)=KA\left[p_1\,(10-10\,(s)^{-1|_9})\right]$$

$$(1+i)(1+e)\frac{hAln}{v_1} = \text{“cost of full steam per hour,” and}$$

$$KA = \text{“cost of engine per hour.”}$$

(See Graphical Method, General Analysis.)

The value of s, which when substituted in equation $2A$ satisfies the equality for the particular conditions of the case, is the ratio of expansion which gives the greatest efficiency of the engine for a given expenditure of money. $^1|_s$ represents the most economical point of cut-off to be employed.

ANALYTICAL METHOD—SPECIAL CASES.

I. STEAM MACHINERY OF U. S. REVENUE STEAMER "RUSH."

Case A^1.—Substituting the values as given and obtained in case a^1, graphical method in equation $2A$ we obtain the equation

$$16.44\,[12009.6\,(s) - {}^{10}_{\;9} - 249.1] = 1.23 \times 12009.6\,[10 - 10\,(s) - {}^1|_9]$$

This equation is satisfied when $s = 5.50$.

Case A^2.—Substituting the values as given and obtained in case a^2, graphical method, in equation $2A$ we have

$$16.44\,[83.4 \times 144\,(s)^{-\frac{10}{9}} - 3.46 \times 144] = 1.23 \times 83.4 \times 144$$
$$[10 - 10\,(s)^{-\frac{1}{9}}].$$

This equation becomes true when $s = 5.07$, which represents the best ratio of expansion to be employed. $\frac{1}{5.07}$ or .197 is the most economical point of cut-off.

Case A^3.—Substituting the values as given and obtained in case a^3, graphical method, in equation $2A$ we have

$$21.231\,[12009.6\,(s.)^{-\frac{10}{9}} - 249.1] = 1.23 \times 12009.6\,[10 - 10\,(s)^{-\frac{1}{9}}].$$

This equation is satisfied when $s = 6.35$, which represents the most efficient ratio of expansion to be employed.

Case A^4.—Substituting the values as given and obtained in case a^4, graphical method, in equation $2A$ we have

$$8.44\,[12009.6\,(s) - \tfrac{10}{9} - 249.1] = 1.23 \times 12009.6\,[10 - 10(s) - \tfrac{1}{9}],$$

which causes s to be equal to 3.84. This value represents the most economical ratio of expansion to be used.

Case A^5.—Substituting the values as given and obtained in case a^5, graphical method, in equation $2A$ we have

$$10.84\,[12009.6\,(s) - \tfrac{10}{9} - 249.1] = 1.23 \times 12009.6\,[10 - 10\,(s) - \tfrac{1}{9}].$$

This equation shows s to be equal to 4.40, which represents the ratio of expansion to be employed for a given expenditure of money.

II. Porter-Allen Engine.

Case B^1.—Substituting the values as given and obtained in case b^1, graphical method, in equation $2A$ we have

$$2.461\ [134.7 \times 144\,(s)^{-\frac{10}{9}} - 16 \times 144] = .293 \times 134.7 \times 144\ [10 - 10\,(s)^{-\frac{1}{9}}].$$

This equation shows s to be equal to 3.30, which represents the best ratio of expansion to be employed for a given expenditure of money.

Case B^2.—Substituting the values given and obtained in case b^2, graphical method, in equation $2A$ we have

$$2.571\ [19396.8\,(s)^{-\frac{10}{9}} - 2304] = .293 \times 19396.8\ [10 - 10\,(s)^{-\frac{1}{9}}].$$

This equation is satisfied when $s = 3.35$, which represents the most economical ratio of expansion to be employed.

Case B^3.—Substituting the values given and obtained in case b^3, graphical method, in equation $2A$ we have

$$3.267\ [19396.8\,(s)^{-\frac{10}{9}} - 2304] = .293 \times 19396.8\ [10 - 10\,(s)^{-\frac{1}{9}}].$$

This equation shows s to be equal to 3.63, which is the best ratio of expansion to be used.

Case B^4.—Substituting the values as given and obtained in case b^4, graphical method, in equation $2A$ we have

$$3.267\ [19396.8\,(s)^{-\frac{10}{9}} - 2448\ ] = .293 \times 19396.8\ [10 - 10\,(s)^{-\frac{1}{9}}],$$

which shows s, the most economical ratio of expansion to be employed, to be equal to 3.55.

III. Ferry Boat Engine.

Case C.—Substituting the values given and obtained in case c, graphical method, in equation $2A$ we obtain the equation

$$8.152\ [4996.8\,(s)^{-\frac{10}{9}} - 432] = .605 \times 4996.8\ [10 - 10\,(s)^{-\frac{1}{9}}],$$

which gives the most economical ratio of expansion to be employed equal to 4.30.

IV. Buckeye Engine.

Case E^1.—Substituting the values given and obtained in case e^1, graphical method, in equation $2A$ we have

$$14391\ [90 \times 144\,(s)^{-\frac{10}{9}} - 17.7 \times 144] = .584 \times 90 \times 144\ [10 - 10\,(s)^{-\frac{1}{9}}],$$

which shows the most economical ratio of expansion to be employed to equal 3.49.

Case $E.^2$—Substituting the values given and obtained in case e^2, graphical method, in equation $2A$ we have

$$14.391\,[12960\,(s) - \tfrac{10}{9} - 720] = .584 \times 12960\,[10 - 10\,(s) - \tfrac{1}{9}],$$

which determines the best ratio of expansion to be employed to be 6.28.

Case E^3.—Substituting the values given and obtained in case e^3, graphical method, in equation $2A$ we have

$$15.459\,[12960\,(6.5) - \tfrac{10}{9} - 720] = .584 \times 12960\,[10 - 10\,(s) - \tfrac{1}{9}],$$

which determines the best ratio of expansion to be used to be 6.49.

V. Corliss (Pawtucket, R. I.) Pumping Engine.

Case D.—Substitute the values given and obtained in case *d*, graphical method, in equation $2A$ we have

$$6.796\,[144 \times 140\,(s) - \tfrac{10}{9} - 3.5 \times 144] = .622 \times 144 \times 140\,[10 - 10\,(s) - \tfrac{1}{9}],$$

which fixes the ratio of expansion to be employed at 4.85.

Case D^1.—Substituting the values given and obtained in case d^1, graphical method, in equation $2A$ we have

$$6.796\,[144 \times 140\,(s) - \tfrac{10}{9} - 3.5 \times 144] = .41 \times 144 \times 140\,[10 - 10\,(s) - \tfrac{1}{9}],$$

which fixes the best ratio of expansion to be employed at 6.09.

CONCLUDING REMARKS.

At the present time but little is known as to the condensation of steam in the cylinder at different ratios of expansion. To use the diagram, and take into account this factor when determined either by experience or by experiment, we must proceed as follows: Assume a given condensation for the probable ratio of expansion for maximum economy and calculate cost of full steam on that basis. We then ascertain ratio of expansion for maximum economy by diagram. If this differs from assumed probable ratio, we allow for a condensation of steam corresponding to the ratio determined by diagram, calculate cost of full steam on this basis, and again find best ratio of expansion by diagram. Two or three trials in this way cause the ratio of expansion found by diagram to correspond exactly with the condensation of steam in cylinder allowed for in calculating cost of full steam. As we may infer

from the practical illustrations of the method and from general analysis, small variations in the amount of condensation of steam at different ratios of expansion do not appreciably affect the best ratio to be employed. But for scientific exactitude, a course similar to that just outlined should be pursued. An exact mathematical expression cannot be found to take into account the variable condensation of steam at different ratios of expansion; but the exact value can be found by the mathematical formulæ, adopting the tentative method just referred to in discussing the course to be pursued in the graphical method, making similar substitutions for "cost of full steam" in the formula. If the ratio of expansion thus found by formula does not correspond to the percentage of condensation assumed in calculating the cost of full steam, allow for a condensation corresponding to the ratio of expansion determined by calculation, and a few trials will cause the condensation of steam assumed in calculating the ratio of expansion to correspond exactly to the ratio determined by the formula.

While it is consistent with our design to thus point out exactly wherein the method presented is mathematically inexact, we can also show that so far as the extent of the variation of the percentage of condensation with different cut-offs is known at the present time, the method is practically independent of such variations. For if from the United States Expansion Experiments, or elsewhere, we examine the amount of the variation of condensation with cut-off, it will be found that a very liberal allowance will be as follows:

Full stroke,	12	per cent.	of total	feed water.
$\frac{1}{2}$ cut-off,	23	"	"	"
$\frac{1}{3}$ "	29	"	"	"
$\frac{1}{4}$ "	36	"	"	"
$\frac{1}{5}$ "	46	"	"	"
$\frac{1}{6}$ "	54	"	"	"
$\frac{1}{7}$ "	62	"	"	"
$\frac{1}{8}$ "	67	"	"	"
$\frac{1}{9}$ "	70	"	"	"
$\frac{1}{10}$ "	74	"	"	"

When these figures are applied, for instance, to the case of the Buckeye engine, treated of in this paper, determining the cost of steam for each particular cut-off given, and estimating in each case the work performed per dollar of expense, it is found that the cut-off, for which this work is a maximum, varies immaterially

from that obtained by the use of the diagram as set forth in case e^1. In the case of the Buckeye engine, the condensation would have the greatest effect of any of the examples cited in this paper, since its cost of full steam is greater in proportion to cost of engine than in any other engine here considered.

It is noticeable by the diagram that the greater the percentage of condensation assumed in any particular engine, the shorter should be the cut-off. This is well known to be contrary to current thought, and the following brief explanation is therefore given to enable the true bearing of the diagram on this point to be understood.

If we assume that there is a constant percentage of condensation for all points of cut-off in any particular engine, then the greater the condensation the more does the influence of the cost of the steam preponderate as compared with the cost of what may be termed the constant elements in any particular problem (interest on plant, wages, oil, etc.), and hence the greater the necessity of reducing the amount of steam used in the engine by cutting off at a less amount of the stroke. This the diagram confirms by showing as it does that the cut-off should be earlier the greater the condensation assumed.

Any law of expansion found by experience to be the correct law for the particular type of engine considered, can be represented by a curve in the same way as the Mariotte law and adiabatic $(\frac{10}{9})$ law of expansion of steam are represented by the curves C D and A B on diagram, and may be used for refinement of accuracy. But we believe it will be found in practice that the actual law of expansion as determined by indicator cards of engines, will vary too little from either the adiabatic curve $(\frac{10}{9})$ law or the Mariotte curve to cause the result obtained by its use to differ appreciably from the results obtained by the use of the curves on the diagram. From a number of cards examined from a variety of steam engines in current use, we find that the actual expansion line varies between the $(\frac{10}{9})$ adiabatic curve and the Mariotte curve.

The method exemplified in this paper is the only scientifically correct one for determining the ratio of expansion which will give the greatest efficiency for a given expenditure of money. It is based on the laws of heat which underlie the use of steam, and upon the laws of cost of production which underlie com-

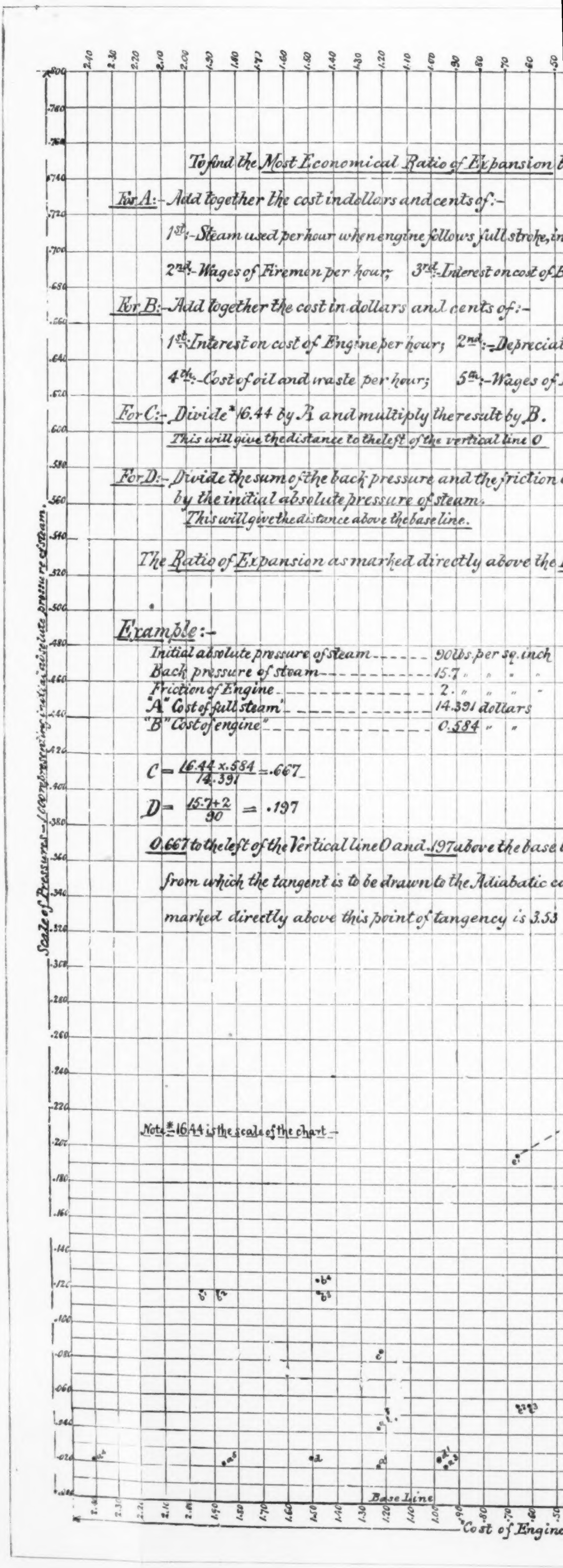
mercial considerations. It is theory in the true sense of the term, for it makes room for all the facts which influence the problem. Could we predict before designing an engine *precisely* what would be the amount of condensation at different ratios of expansion, and the law of expansion of steam in the cylinder under given conditions, we could predict *precisely* what would be the best ratio of expansion. For the other conditions, cost of fuel, cost of boiler and engine, attendance, etc., are at any time practically fixed quantities at particular localities. It is here that we still need considerable experimental research and much experience.

We have found, however, in our examples, and infer from general analysis, that slight variations in any of the quantities which enter the solution of the problem, do not materially affect the most economical point of cut-off. The best ratio of expansion, important as it is, is fortunately not a sensitive quantity, and even slight variations from the best ratio do not appreciably influence the actual economy. But heretofore empirical formulæ have been employed which in many cases gave results differing widely from the truth, and it was always impossible to say whether they so differed or no, except by the institution of practical test and experience too expensive and difficult to secure. If in adopting the above method, we may occasionally be unable to fix some conditions precisely, we can determine the limit of error in the result attained, and can strive to supplement the knowledge we are in need of. Tho adoption of a true method of solution means progress; the adoption of empirical methods, except where they can be constantly checked by, or where they are approximations to, scientific methods, means stand-still. Empirical formulæ are never more than make-shifts. They are sometimes necessary evils,—exerting often much temporary good,—but they are always evils, inasmuch as they do not encourage new research. We live and strive for progress. It is for you, gentlemen, manufacturers and users of steam-engines, to say whether we will adopt the true, even if it means a little work, or if we will accept the false or doubtful, which means no work at all. No work but no truth! No work but no progress!

APPENDIX No. 1.

(No Allowance made for Condensation of Steam.)

Let a volume $AB = v_1$ expand along isothermal BE. Then according to theorem, Art. 239, Rankine's Steam-engine, the heat


To find the Most Economical Ratio of Expansion
For A:- Add together the cost in dollars and cents of:-
1st:- Steam used per hour when engine follows full stroke,
2nd:- Wages of Firemen per hour; 3rd:- Interest on cost of
For B:- Add together the cost in dollars and cents of:-
1st:- Interest on cost of Engine per hour; 2nd:-
4th:- Cost of oil and waste per hour; 5th:- Wages of
For C:- Divide* 16.44 by A and multiply the result by B.
This will give the distance to the left of the vertical line O
For D:- Divide the sum of the back pressure and the friction
by the initial absolute pressure of steam.
This will give the distance above the base line.
The Ratio of Expansion as marked directly above the
Example:-
Initial absolute pressure of steam ------ 90 lbs per sq. inch
Back pressure of steam ------------- 15.7
Friction of Engine ------------------ 2.
"A" Cost of full steam ------------- 14.391 dollars
"B" Cost of engine ----------------- 0.584
C = 16.44 x .584 / 14.391 = .667
D = 15.7+2 / 90 = .197
0.667 to the left of the Vertical line O and .197 above the base
from which the tangent is to be drawn to the Adiabatic
marked directly above this point of tangency is 3.53
Note * 16.44 is the scale of the chart.
Scale of Pressures - (Corresponding initial absolute pressure of steam.)
Base Line
Cost of Engine

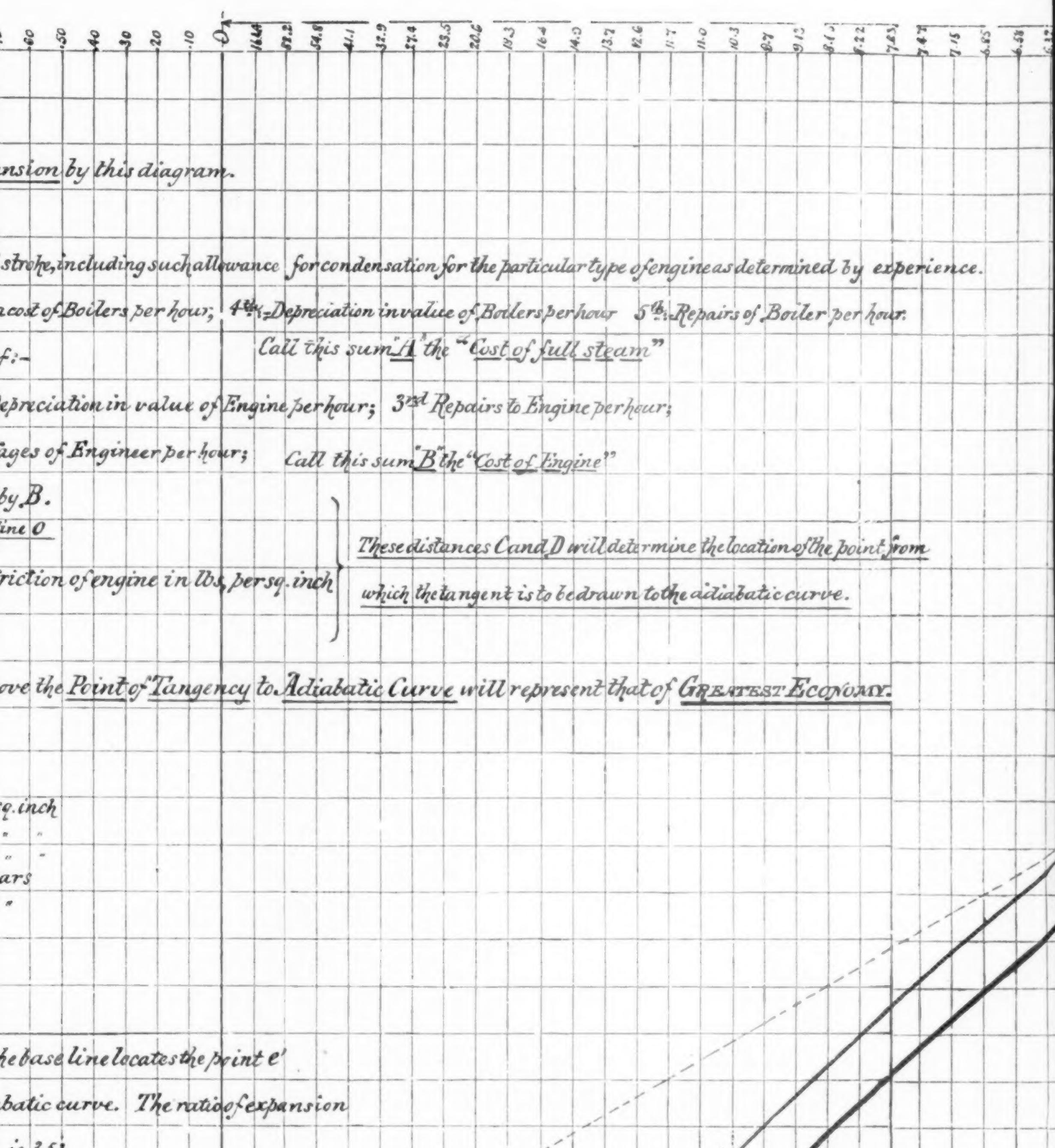

.60 .50 .40 .30 .20 .10 0
164.4 82.2 54.8 41.1 32.9 27.4 23.5 20.6 18.3 16.4 14.9 13.7 12.6 11.7 11.0 10.3 9.7 9.13 8.65 8.22 7.83 7.47 7.15 6.85 6.58
nsion by this diagram.
stroke, including such allowance for condensation for the particular type of engine as determined by experience.
n cost of Boilers per hour; 4th - Depreciation in value of Boilers per hour 5th - Repairs of Boiler per hour.
Call this sum "A" the "Cost of full steam"
f:-
epreciation in value of Engine per hour; 3rd Repairs to Engine per hour;
Vages of Engineer per hour; Call this sum "B" the "Cost of Engine"
by B.
ine O
riction of engine in lbs. per sq. inch
These distances C and D will determine the location of the point from which the tangent is to be drawn to the adiabatic curve.
ove the Point of Tangency to Adiabatic Curve will represent that of Greatest Economy.
q. inch
ars
he base line locates the point e'
batic curve. The ratio of expansion
is 3.53
C
A
To apply the diagram to the design of an engine to supply a
water consumption and clearance for different rat
I - Determine the range of sizes of engines which
steam pressure may be given by a - 30"
by a 20"
" an 18"
II - To the sizes selected, apply the method of the d
III - Select the sizes for which the most econom
determine the water consumption corresp
IV - Add together the cost of each engine per hors
Then other things being equal, the size
.60 .50 .40 .30 .20 .10 0 .10 .20 .30 .40 .50 .60 .70 .80 .90 1.00 1.10 1.20 1.30 1.40 1.50 1.60 1.70 1.80 1.90 2.00 2.10 2.20 2.30 2.40 2.50
"Cost of Engine per hour" — "Cost of Steam per hour"

Fig. 58.

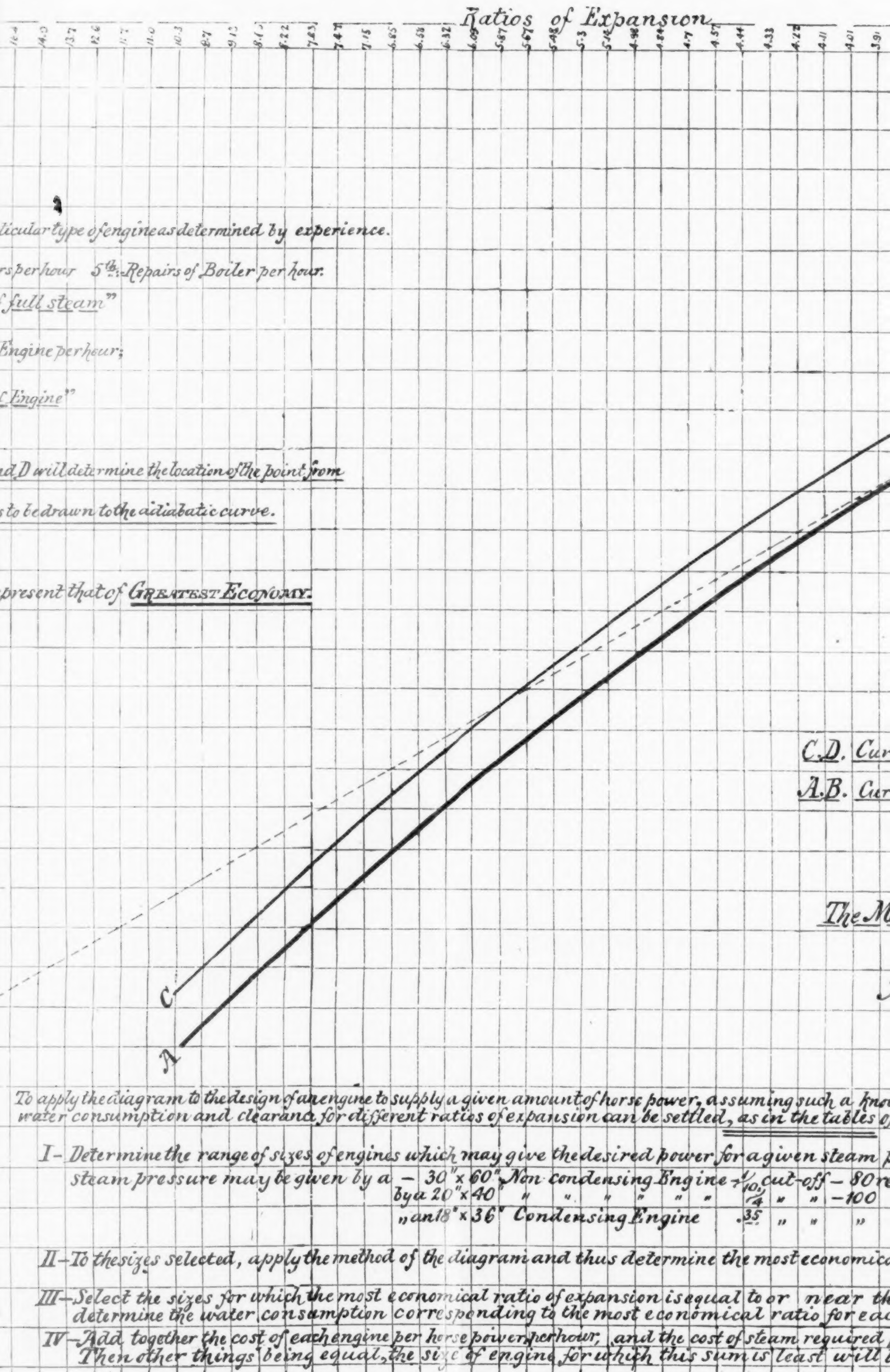
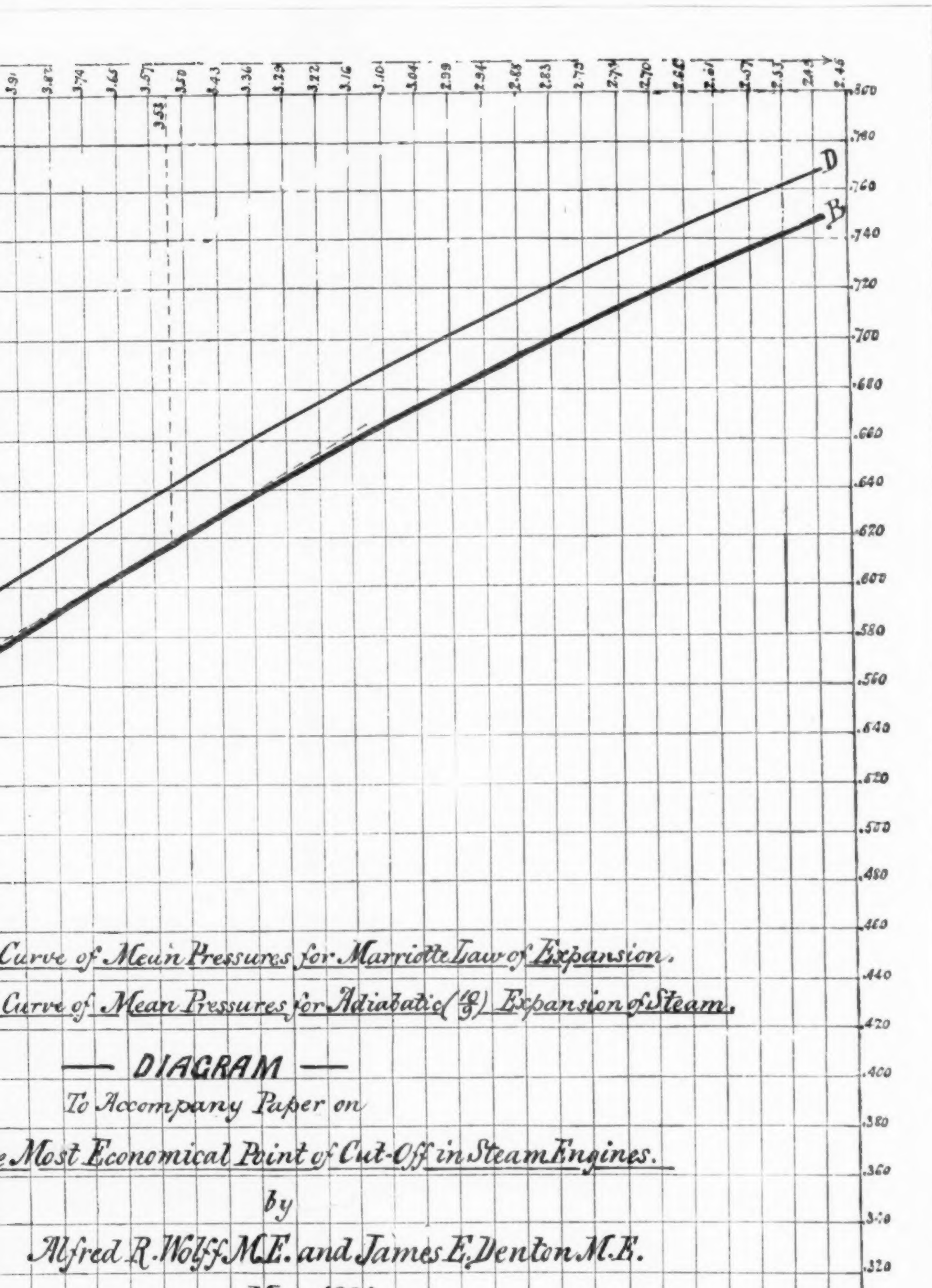

Ratios of Expansion
ticular type of engine as determined by experience.
rs per hour 5th: Repairs of Boiler per hour.
full steam"
Engine per hour;
Engine"
nd D will determine the location of the point from
s to be drawn to the adiabatic curve.
present that of GREATEST ECONOMY.
C.D. Cur
A.B. Cur
The M
C
A
To apply the diagram to the design of an engine to supply a given amount of horse power, assuming such a kno
water consumption and clearance for different ratios of expansion can be settled, as in the tables o
I– Determine the range of sizes of engines which may give the desired power for a given steam p
steam pressure may be given by a – 30"×60" Non condensing Engine 1/10 cut-off – 80
by a 20"×40" " " " " " 1/4 " " –100
" an 18"×36" Condensing Engine .35 " " "
II– To the sizes selected, apply the method of the diagram and thus determine the most economica
III– Select the sizes for which the most economical ratio of expansion is equal to or near th
determine the water consumption corresponding to the most economical ratio for eac
IV– Add together the cost of each engine per horse power per hour, and the cost of steam required
Then other things being equal, the size of engine for which this sum is least will

Fig. 58.


D
B
Curve of Mean Pressures for Marriotte Law of Expansion.
Curve of Mean Pressures for Adiabatic (10/9) Expansion of Steam.
— DIAGRAM —
To Accompany Paper on
e Most Economical Point of Cut-Off in Steam Engines.
by
Alfred R. Wolff M.E. and James E. Denton M.E.
May 1881.
knowledge of the type of engine to be built that the mean effective pressure,
les of engine performances given in the catalogues of Modern Engine Builders.
m pressure and piston speed. For example – 200 Horse Power at 65 lbs.
80 revolutions ; by a 22" x 44" Non-condensing Engine 1/2 cut-off – 100 rev's ;
00 " " " ; an 18" x 36" " " " " " " 1/2 " " – " "
" " " " ; &c ; &c.
mical ratio of expansion for each size.
r the ratio necessary to the development of the required power and
each size.
red per horse-power per hour for its most economical ratio of expansion.
ill be the cheapest engine to supply the given horse-power.

which must be supplied from an external source is *BEH* where *H* is the point in which the adiabatic curves *BG* and *EH* meet. If we adopt the law $p\,v^{\gamma} =$ constant for the adiabatics *BH* and *EH*, *H* is at infinity; for $p_1 v_1{}^{\gamma} = C$ being the equation of *BG* and

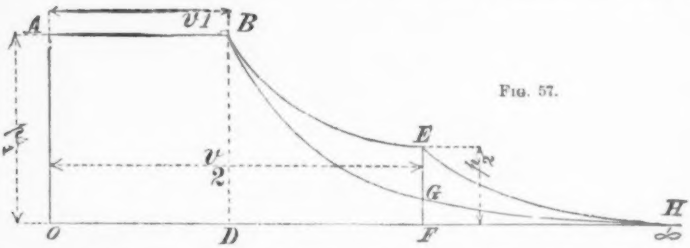


FIG. 57.

$p_2 v_2{}^{\gamma} = C_1$ the equation of *EH* we must evidently have for their point of intersection $p_1 = p_2$ and $v_1 = v_2$ when C must equal C_1; but this latter equality between the unequal quantities C and C_1 can only be mathematically true for $p = o$ or $v = \infty$ whence *H* is an infinity. Now if *H* be at infinity then

$$BDH = \int_{v_1}^{\infty} p\,dv \text{ and } EFH = \int_{v_2}^{\infty} p\,dv. \quad \text{Hence } BEH = BEFD + \int_{v_2}^{\infty} p\,dv - \int_{v_1}^{\infty} p\,dv.$$

To integrate we put for p in the first integral $p = \frac{p_1 v_1^{\gamma}}{v^{\gamma}}$ and in the second $p = \frac{p_2 v_2^{\gamma}}{v^{\gamma}}$.

So that we have

$$BEH = BEFD + \left[\frac{p_1 v_1^{\gamma}}{1-\gamma} v^{-\gamma+1}\right]_{v_1}^{\infty} \left[-\frac{p_2 v_2^{\gamma}}{1-\gamma} v^{-\gamma+1}\right]_{v_2}^{\infty}$$

$$= BEFD + \frac{p_1 v_1^{\gamma}}{1-\gamma}\left(\infty^{-\gamma+1} - v_1^{-\gamma+1}\right) - \frac{p_2 v_2^{\gamma}}{1-\gamma}\left(\infty^{-\gamma+1} - v_2^{-\gamma+1}\right)$$

but since γ is greater than 1. $\infty^{-\gamma+1} = \frac{1}{\infty^{\gamma-1}} = 0$ whence

$$BEH = BEFD + \frac{p_2 v_2^{\gamma}}{1-\gamma} v_2^{-\gamma+1} - \frac{p_1 v_1^{\gamma} v^{-\gamma+1}}{1-\gamma} = BEFD + \frac{p_2 v_2}{1-\gamma} - \frac{p_1 v_1}{1-\gamma}.$$

But since $p_1 v_1$ and v_2 lie in an isothermal $p_1 v_1 = p_2 v_2$ whence $BEH = BEFD + 0 = BEFD$.

APPENDIX No. 2.

(No Allowance made for Condensation of Steam.)

If the expansion curve is an isothermal we have $p_1 v_1 = p_2 v_2$, and the work proper equals $p_1 \frac{v_2}{s} + p_1 \frac{v_2}{s} \log^e s - p_2 v_2$, where p_1, v_1, p_2 and v_2 have values as above and $s =$ ratio of expansion or $\frac{v_2}{v_1}$. The

expenditure for this work is the value of steam up to point of cut-off $\left(\frac{Cv_2}{s}\right)$ + value of cut-off area as per previous appendix $\left(C_3 p_1 \frac{v_2}{s} \log_e s\right)$.

[If we make the unit of value dollars, then if N be the price of coal per ton and 10,000 heat units be the useful heating effect of a pound of coal, C will equal

$$\frac{\textit{Total heat of evaporation of one cubic foot of steam of pressure } p_1 \times N}{10{,}000 \times \textit{lbs. in ton of coal.}}$$

$$C_3 \text{ will equal } \frac{1}{772} \times \frac{1}{10{,}000} \times \frac{N}{\textit{lbs. in ton}}\Bigg].$$

The efficiency of the steam (y) equals

$$\frac{p_1 \frac{v_2}{s} + p_1 \frac{v_2}{s} \log_e s - p_2 v_2}{\frac{Cv_2}{s} + \frac{C_3 p_1 v_2}{s} \log s} = y = \frac{p_1 + p_1 \log_e s - p_2 s}{C + C_3 p_1 \log s}$$

To obtain a maximum value of y we have

$$\frac{dy}{ds} = \frac{\left(\frac{p_1}{s} - p_2\right)\left(C + C_3 p_1 \log_e s\right) - C_3 \frac{p_1}{s}\left(p_1 + p_1 \log_e s - p_2 s\right)}{(C + C_3 p_1 \log_e s)^2} = 0$$

whence

$$\frac{Cp_1}{s} - Cp_2 + \frac{C_3 p_1^2 \log_e s}{s} - C_3 p_2 p_1 \log_e s - \frac{C_3 p_1^2}{s} - \frac{C^3 p_1^2 \log_e s}{s} + C_3 p_1 p_2 = 0$$

$$C\left(\frac{p_1}{s} - p_2\right) - C_3 p_1 \left(\frac{p_1}{s} - p_2\right) - C_3 p_1 p_2 \log_e s = 0$$

or

$$\left(\frac{p_1}{s} - p_2\right)\left(C - C_3 p_1\right) C_3 p_1 p_2 \log_\varepsilon s = 0$$

Appendix No. 3.

(No Allowance made for Condensation of Steam.)

Let p_1 = initial absolute pressure of steam.

Let p_2 = back pressure.

Let p = terminal pressure.

Let v_1 = initial volume.

Let v_2 = volume at end of expansion assuming release not to occur until the end of the stroke.

Let s = the ratio of expansion.

Let the expansion be adiabatic according to the approximate formula $p\ v^\gamma$ = Constant. The useful work for any ratio of expansion will be

$$p_1 v_1 + \int_{v_1}^{v_2} p\,dv - p_2 v_2 = p_1 \frac{v_2}{s} + \frac{p_1 v_1^\gamma}{1-\gamma}\left[v_2^{1-\gamma} - v_1^{1-\gamma}\right] - p_2 v_2 = p_1 \frac{v_2}{s} + \frac{p_1}{1-\gamma}$$

$$\left(\frac{v_2}{s}\right)^\gamma \left[v_2^{1-\gamma} - \left(\frac{v_2}{s}\right)^{1-\gamma}\right] - p_2 v_2 = p_1 \frac{v_2}{s} + \frac{p_1}{1-\gamma} \frac{v_2}{s^\gamma} - \frac{p_1}{1-\gamma} \frac{v_2}{s} - p_2 v_2.$$

The cost will be proportional to the initial volume or Constant $\times v_1 = \frac{Cv}{s}$. So that the efficiency will be proportional to

$$\frac{p_1 \frac{v_2}{s} + \frac{p_1}{1-\gamma} \frac{v_2}{s^\gamma} - \frac{p_1}{1-\gamma} \frac{v_2}{s} - p_2 v_2.}{\frac{Cv_2}{s}}$$

Let this fraction equal y. Then

$$y = \frac{p_1 v_2 + \frac{p_1}{1-\gamma} \frac{v_2}{s^{\gamma-1}} - \frac{p_1}{1-\gamma} v_2 - p_2 v_2 \, s.}{Cv_2}$$

and for a maximum we have

$$\frac{dy}{ds} = p_1 (s)^{-y} - p_2 = 0 \text{ whence } s^\gamma = \frac{p_1}{p_2}.$$

Substituting this value of s^γ in the relation of $p_1 v_1 = pv^\gamma = ps^\gamma v_1{}^\gamma$ we have $p_1 = \frac{p_1}{p_2}$ when $p = p_2$.

Appendix No. 4.

If we assume the law of expansion of steam in cylinder to be Mariotte's law, but still accepting adiabatic expansion of steam, we have $Z = 1 + log_e s$; useful work performed by engine per hour equal $p_1 \, Aln \, (1 + log^\varepsilon s) - p_2 \, Aln$ and the efficiency of the engine for a given expenditure of money equals

$$y = \frac{p_1 \frac{Aln}{s} (1 + log_\varepsilon s) - p_2 \, Aln}{\frac{hAln}{sv_1} + KA}$$

To obtain a maximum value of y we have

$$\frac{dy}{ds} = \frac{d \left[\frac{p_1 \frac{Aln}{s} (1 + log_\varepsilon s) - p_2 Aln}{\frac{hAln}{sv_1} + KA} \right]}{ds} = 0,$$

which reduces to

$$KAp_1 \, log_\varepsilon s = \frac{hAln}{v_1} \left[\frac{p_1}{s} - {}_2d \right] = \text{Equation 3.}$$

In determining Equation 3, for the sake of convenience in mathematical demonstration, no allowance has been made for clearance and condensation of steam in cylinder. In actual practice there is clearance and condensation occurs. Allowance for this condensation is made in Equation 3, by adding such a percentage for

condensation to the term ($\frac{hAln}{v_1}$ + steam used for clearance) as determined by experience for the particular ratio of expansion in the particular engine considered. Thus letting i = percentage of clearance (in decimal numbers) of stroke capacity of cylinder, and c = percentage of steam (in decimal numbers) used in cylinder above that called for by the value $\frac{Aln}{v_1}(1+i)$ which latter expression represents amount of steam used for full stroke if there is no condensation, then Equation 3 becomes

$$KAp_1 \log_\epsilon s = (1+c)(1+i)\frac{hAln}{v_1}\left[\frac{p_1}{s} - p_2\right] = \text{Equation } 3A.$$

$(1+i)(1+c)\frac{hAln}{v_1}$ = "cost of full steam per hour," and KA = "cost of engine per hour." (See Graphical Method, General Analysis; and foot note on page 170.)

The value of s, which when substituted in Equation $3A$ satisfies the equality, is the ratio of expansion for maximum economy under the requirements of the assumption of this appendix.

Substituting the values given in Case a^1, "graphical method, special cases," in Equation $3A$, we have the equation

$$1.23 \times 83.4 \times 144 \log_\epsilon s = 16.44\left[\frac{144 \times 83.4}{s} - 1.73 \times 144\right]$$

which is satisfied by a value of $s = 6.31$. If we neglect the constant "cost of engine," KA, we have $KA = 0$, which changes Equation $3A$ to the form $s = \frac{p_1}{p_2}$ (Equation 4), corresponding with Professor Marks' result for efficiency of fluid alone, under the assumption of this appendix and with the restrictions of previous comments in this paper on Professor Marks' work.

While 6.31 is the best ratio for Case a^1 when "cost of engine" is taken into account, substitution in Equation 4, where such cost is neglected, would cause s to become $\frac{83.4}{1.73} = 48.21$.

XXXV.

DISCUSSION

OF

THE RATIO OF EXPANSION AT MAXIMUM EFFICIENCY.

BY R. H. THURSTON, PROF. MECH. ENG., STEVENS INST. TECH., HOBOKEN, N. J.

AND

THE MOST ECONOMICAL POINT OF CUT-OFF IN STEAM-ENGINES.

BY ALFRED R. WOLFF, M.E., NEW YORK CITY. AND JAMES E. DENTON, M.E., HOBOKEN, N. J.

MR. CHARLES E. EMERY: Mr. President, I hardly expected to be called upon to say anything this evening, and yet as my name has been mentioned, although suffering from a rather severe indisposition, I will try to say a few words.

The subject is one of great interest to us all—one of special interest to me. Some years ago I was enabled to make a very extended series of experiments upon the general subject of the economy of steam, of which only a portion has been published. My time has been too much occupied to enable me to discuss the matter properly. As a mere incidental feature of the analysis of the experiments with the Government steamers of the Revenue Service, of which I was the superintending and designing engineer,—as a mere incidental matter, I say,—I put in an empirical formula referring to this general subject, viz., the most economical point of cut-off. It was not, as my very able former assistant supposed, founded entirely upon those experiments, but upon many others which I had myself made, and upon the investigation of a number of persons in the same line,—experiments made by Mr. Isherwood and others under his general direction, and particularly upon a very elaborate series of experiments made at the Novelty Iron Works to test this very subject, in connection with the discussion that arose with relation to the steamers constructed during the war under Mr. Isherwood's direction. It will be remembered that Mr. Isherwood took the ground that in no case was extreme expansion warranted. We know that some of his opponents took

precisely the opposite ground—that we could reduce the cost of power in a steam-engine to a very low limit by simply enlarging the size of the cylinder and expanding to a very large degree. Both parties I fear were very much like a horse with blinders; they could see simply in front, the direction to which they were pointed. Both were right to a limited extent and in a narrow groove.

Undoubtedly there have been, by very many who have discussed the subject since, theories propounded, mathematical solutions proposed, which more nearly reached to the root of the whole problem. That is, they took hold of it in a more scientific way to try and embody all the conditions which entered into its consideration. If we were to consider, in relation to the motions of the planets about the sun, simply centrifugal force, the planets would go off in straight lines and we should never see them again. If on the contrary we considered the centripetal force—the force of attraction alone—they would fall into the sun and add to its heat-giving power. The two must be considered together; and to make a full consideration of the problem you necessarily bring in the element of the resistance of possibly the luminiferous ether, or matter of that kind in space. In this case I have only mentioned three conditions. In a steam-engine you know that there are very many. Some were mentioned in one paper to-night that were not mentioned in the other; and both sets of conditions were entirely proper. All should be considered; all should be weighed. This subject is not to be settled by merely knocking a chip off one's shoulder; by saying that an experiment made to settle partially a subject proves one's entire experience in error. It is not to be settled by anybody's assumption or by anybody's opinion; it is not to be settled by formulæ. Both papers acknowledge a want of information necessary to fill in the constants and make their formulæ exact in every important particular. Usually the formulæ given are at the best empirical formulæ. If any one part is left out the rest is empirical; and it is to be remarked that very much of all we are taught in the scientific world is founded on empirical formulæ, which in some cases are not stated to be such. The index of the power, in the formula for compression is simply empirical—derived from experiment. It has been found by trial that the compression curve about follows the curve which would be developed by raising the pressure to a power represented by the index. So in every one of these cases

there will be somewhere some particular place that has to be filled in with a quantity derived from experiment. It has been my effort, and both papers in some degree speak of the necessity of such effort on the part of all, to fill in this data in order that we may know just exactly what we are doing. The only question that arises is whether we do not get at the results just as accurately by compiling the final facts themselves as by using them to fill in constants in certain formulæ. Now, no one would be delighted more than myself to see this subject mathematically demonstrated. If one could trace either of these operations mathematically with as great clearness as the cycle of operations was presented by our worthy President in language; if the change in the curve produced by condensation due to certain differences in temperature between the walls and the ingoing and outgoing steam could be fixed by an exponential or other formula; if in addition to that we could bring into that formula just the changes that were due to the shape of the ports; the changes that were due to the mass of the metal of the cylinder, changes due to opening the doors and allowing the cold air to blow in on parts of the cylinder, and changes due to a hole in the wall somewhere where the flange of the steam pipe was not protected from radiation, etc.—if all these changes could be formulated and brought out so that we could accurately calculate their influence as we sit in our offices, it certainly would be delightful. But it is impossible. We must depend upon the results themselves; and I cannot recommend anything to the energetic gentlemen who have so industriously presented a paper really of interest and of great value, containing very many of the elements to be considered, and very properly considered—I cannot suggest to them and to others anything better than that they get together all the evidence available on this subject, digest it and give us the mean results, classifying the different kinds of engines, the different conditions with which they operate, the different qualities of steam—if they can only find them out, and I do not know how they can in advance of experiment—and give us some sort of formula by which we can ascertain somewhere near what the results should be; if instead of merely theoretical results, which must be first used in order to settle the general form of the equation, they also give us compilations of what has been done in the experimental line throughout the world, we shall be nearer the solution of this and very many problems in steam engineering.

These papers are so elaborate that it would be impossible to review them in any sense in a reasonable time as respects details. They contain much that is of great value, many suggestions which will help us all, and are prominent steps towards the solution of the problem. The whole point turns on the fact that in any investigation we must include all the conditions. Starting out with the paper of our worthy President we first consider the expansion of the steam and back pressure. Touching the next point I do not quite agree either with him or the writers of the other paper in regard to friction. The friction does not necessarily increase to any grave extent with the size of the engine doing the same work. It has more to do with the work done than with the size of the engine. The increased friction is only that due to the extra weight of the parts and the extra friction of the piston ring, so that will be taken too large if fixed at so many pounds per square inch of piston and therefore increasing with the size of the engine, as is usual, which perhaps they do not wish to imply, but still in the hasty reading I could not tell. We must consider also the internal cylinder condensation, which I must say with others, I have tried to impress upon the minds of engineers for many years. We must consider also the subject of external protection, which is very important; then the subject of piston speed; then the subject of details to make the apparatus reliable; then the subject of cost, which is perfectly legitimate; but it is only the interest that affects the problem, and interest at the low rates at present does not count very fast, so that it is not such a wonderful find after all, though it has something to do with the subject; finally, as a matter of course, we must consider the fuel. But in the papers to-night necessarily there have been some assumptions as to the condensation, instead of reference to a curve which will work them out according to conditions, and those assumptions make all the formulæ, no matter how accurate they are claimed to be, simply empirical formulæ, nothing more. There are several points which have not been mentioned too; one of them occurs to me. In marine work particularly, and to a great extent in other work, if we fix the size of the engine for the exact ratio of expansion calculated to secure economy, which in later days would be smaller than formerly, as the boilers get old, we should not have a proper sized cylinder to work off the low pressures of steam necessary to carry. In factory work, as a matter of course, it would be economy to renew the boilers when they could not maintain the original

pressure. In marine work we cannot always do it. That point has to be considered. I would, however, repeat a statement I have made previously to this Society, that the tendency of my practice is to reduce the size of engines as much as possible, and obtain the power with a reduced pressure. I have found that expansion has been over-done in many, many cases. As I said at the last meeting, in the case of the "Rush" with only six expansions, the cost of feed water per horse power per hour was eighteen and four-tenth pounds. With an engine very much larger at the Lawrence Pumping Works, Mr. Leavitt obtained the horse-power with sixteen pounds. His largest cylinder was thirty-eight inches in diameter and nine feet stroke, and my largest cylinder was thirty-eight inches in diameter and twenty-seven inches stroke. In one case all the elaborate details necessary to secure economy were employed, in the other simply slide valves. There were an entirely different set of conditions and entirely different ratios of expansion in the two cases. The smaller engine developed the greater power, and with an expansion of only about six; the larger engine had an expansion of some fourteen, I think it was, and there was a difference in the proportion of sixteen to eighteen in favor of the larger engine, which cost undoubtedly several times as much as the smaller one, which is to be considered in comparing the general results. I was much gratified with the first of Professor Thurston's tables, as it expresses very closely what my experience shows me will be found in practice. In regard to the second table I have thought that good practice would reduce the quantities all the way through.

THE PRESIDENT: Good practice would make them ten per cent. lower evaporation.

MR. EMERY: I am speaking of water per horse-power per hour which would not be affected by the boiler at all. At these higher expansions I should not expect to do very much if any better than at the medium one—11 or 12. You will see opposite 6 here 22. In the compound engine 18 was obtained, and with the jacketed single engine 20 instead of 22, and the compound 18,—quite a difference. At 14 expansions, the tabular result is exactly as it was in the case of the Lawrence engine that I was speaking of. My impression is that the Lawrence engine would do better if speeded up so as to get more power out of it and less expansion. I have thought that if it had smaller cylinders it could have done more work without increasing the cost of the power at all, and proba-

bly have somewhat diminished it. At slow piston speeds the condensation in the cylinder balances the gain due to the expansion. I can only repeat the necessity of settling all these problems by a compilation of the various experiments on this subject, and an examination of the results may help us to prepare a formula which will enable one to design engines, and vary the size of an engine without being obliged to do it by the rule of thumb.

Mr. A. R. Wolff: There are a number of points which the last speaker, my esteemed friend Mr. Emery, referred to, that I would like to call attention to. The first thing that I desire to insist upon very strongly is, that the facts presented in our paper are not all empirical, and that the whole discussion is entirely one which in my sense of the term comes within the province of true theory. It allows for that portion of the problem which relates directly to the question of efficiency of fluid, and it takes into consideration all the commercial considerations which should enter. The only point of which it takes no account—but it does take account of it in the analysis, it only does not take account of it in all of the practical illustrations—is the question of variable condensation at different ratios of expansion; and of that we have not the slightest definite knowledge. I agree with Mr. Emery entirely, that no experiment to this day has been made that will permit us with the slightest scientific approximation to the truth, to say what in a particular style of engine will be the condensation of steam cutting off say at $\frac{1}{4}$, $\frac{1}{3}$, or $\frac{1}{8}$, etc. We do not know what effect different ratios of expansion in the same engine will have upon the question of consumption of steam. This is a very important question indeed, and I trust sincerely that very soon elaborate experiments will be made to answer it satisfactorily. On the other hand I think as we have shown very clearly in our paper, that as far as the problem we discuss is concerned it is of secondary importance. This question of efficiency of fluid is actually of great importance—because in scientific matters in trying to get at the facts we want to get them as close as possible. To me the difference between a ratio of expansion of 5 or a ratio of expansion of 6, is a very important difference; it may not make so much difference in the actual economy; but I want to know if possible whether it is 6 or 5. We have shown in our paper, that a variation of condensation of thirty per cent. does not change the most economical ratio of expansion from 5 to 6. Therefore we might justly claim, if we so chose, that our results were almost scientifically correct. But to

us the idea of scientifically correct means exactly correct. It does not mean within fifteen per cent. or ten per cent., but it means within less than one per cent. But what application would our formula have in actual practice? Say we assume a condensation of thirty per cent. for a particular engine. Very well; we find the best ratio of expansion according to our method, we use it in our engine, and we find an actual condensation of twenty-five per cent. Then we say the condensation is twenty-five per cent. in this case, and not thirty per cent. Therefore we will find the ratio of expansion allowing for a condensation of twenty-five per cent. Practically we find that that makes no or very little difference. Probably the valves will not have to be changed at all; probably the result would come within three per cent. say of the previously determined ratio of expansion; but if we can we desire to narrow it down from this three per cent., and in that respect I heartily indorse Mr. Emery's remarks that we need careful experiments, just as heartily as I indorse the correctness of the method presented in our paper. I believe that empirical methods are often most dangerous methods, just as in this particular case of the "cut-off" problem we have seen empirical formulæ given by such men as Professor Thurston and Mr. Emery, carry with them a weight which, if the gentlemen themselves did not desire to convey, the engineering public has accorded them. If we ask any manufacturer of steam-engines to-day on what formula he would calculate the best ratio of expansion for his steam-engine, he would give us either the formula of Professor Thurston or that of Mr. Emery. He would not cite Professor Marks' formula, because his is so evidently erroneous; but Professor Thurston's formula and Mr. Emery's formula seem to agree pretty closely with what has been considered good practice. But we have seen, for instance in the case of the "Rush," that according as she is on the Pacific coast or as she is on the Atlantic coast, there should be used entirely different ratios of expansion; the differences amounting to between 5.50 and 3.84 in one example. To me these two ratios do not appear as close to each other, though it may appear so to others. Now I desire to refer to some other matters that Mr. Emery mentioned. He said the formula which he had given had been based not alone upon experiments which have been recorded in the *Journal of the Franklin Institute* but upon some other experiments, and if any one will refer to the papers in the *Journal of the Franklin Institute*, I think it will be very difficult for him to get

any impression, but that the formula was deduced from the tables and results recorded in the *Journal of the Franklin Institute.* On the other hand, knowing that matters of this kind are usually not presented so accurately as they ought to be, Mr. Denton and myself have taken the liberty of writing to Mr. Emery, asking him how he obtained his formula. He referred us to some other experiments besides—some incomplete experiments—to the experiments made at the Novelty Iron Works and others. We have examined those experiments as carefully I think as any person can, and I cannot see the slightest connection between the results of the Novelty Iron Works (Isherwood's) experiments as recorded in *Weisbach's Mechanics* edited by Dubois & Buel and Mr. Emery's formula. In fact it is perfectly astonishing how the water consumption varies with the expansion. I should defy anybody to even deduce an empirical formula from the results recorded there. Any reference given in that letter of Mr. Emery we have looked into, and we have failed to see any connection between such experiments and his formula. Still we were very careful to put in our paper that his deductions were apparently based—and not that they were actually based—on the tables of the *Journal of the Franklin Institute.*

Mr. Emery referred to our paper as if we took no account of the question of efficiency of fluid. Well I think myself it is difficult in merely listening to a paper to get all the conditions that have entered; and I feel confident that when Mr. Emery reads our paper and goes through the mathematical solution which makes room for every question of this kind, he will see that this comment of his was in error. I prefer not to enter into this question any more deeply today except by putting in a definite denial of the correctness of that assertion; especially as I believe Mr. Emery will see his mistake as well when he reads our paper. I only desire to put myself here in the minutes on record as stating that the formula given in our paper and that the graphical solution—in short the method we present—admits of every single condition entering which can affect the economy. Our practical illustrations may not have taken into account every such condition, but the method shown there enables the engineer to take into consideration every condition which influences the problem, if he has those conditions at hand. On the other hand I desire to place ourselves on record as saying that practically we find that those conditions which as a matter of efficiency of fluid are of the greatest possible interest to us, that is

to say, the question of exact amount of condensation of steam in the cylinder and the precise law of expansion of steam in the cylinder will not materially affect the most economical point of cut-off as we have shown it here in our paper. There are other problems in which this determination would be of greater importance, and I indorse with pleasure Mr. Emery's remarks as to the necessity of accurate experiments, and while I for one should be delighted to take part in making experiments of that kind, I should certainly feel that at best I would only reduce our limit of error, perhaps from the percentage of five (5) which may now exist to the percentage of zero (0). Relative to the factor "friction" I think that friction as taken into account in our paper has received exactly its due consideration. We have shown that the useful work is the mean pressure on one side of the piston into the area of the piston less the back pressure. Now the back pressure of steam is one item in that back pressure, and the other item in the back pressure is friction, which practically acts as a constant resistance to the forward stroke of the engine and to the work of the engine, and, therefore, if we have one pound per square inch of friction, it means that we need to have that much more work on the forward side of the cylinder in order to overcome that friction and still accomplish the desired result. The question of friction in steam-engines enters exactly the same way as back pressure of the steam.

As a matter of interest I should like to call attention to the case of the Pawtucket engine, which to me was a very interesting problem indeed, because in this case it became apparent that while 17 might be the best expansion for efficiency of fluid, 5 was the best ratio for actual economy. Now let us assume 100 per cent. condensation in this engine, and we see that it does affect the ratio perhaps by increasing the ratio of expansion from somewhere near 5 to 7. By the use of the diagram this can be seen. I believe this meets most of the points which Mr. Emery has referred to.

Mr. Emery: I have only a word to say. The parties misunderstood me here in supposing that I stated that the former gentleman did not take into account the condensation and also the action of the steam in the cylinder. I do not recollect saying anything of the kind. It can easily be shown by the record, and I should be happy to take it out, if there is anything of the kind in the report, as I did not intend it. I must add that Mr. Wolff is very

much at fault in his recollection of my statements on the subject, in the *Journal of the Franklin Institute* for 1875. I am sorry he has not made complete quotations, and as the copy of the *Journal* does not appear to be here I will ask the Secretary to add a quotation to these remarks when printed. I feel assured that I would adhere now closely to what I said then, though of course with the same qualifications.* There is one more point that I omitted that naturally comes in in response to the reply of the gentleman that last spoke. Any one who has had an extensive experience with experiments finds himself baffled by very great differences between experiments in the same series, and it needs considerable investigation to find what causes can operate to produce differences when apparently the conditions are the same. In all cases it will be found that there are differences in conditions, and only the one making the experiment is competent really to judge what those differences are and know which one of the series of experiments is the one which should be taken as the average of the better practice. I recollect that after working three days on

* Extract from paper by Mr. Emery, on Compound and Non-Compound Engines, in the *Journal of the Franklin Institute*, May, 1875, page 334.

"It is not practicable, with the information available (many experiments not having been put in shape for comparison), to calculate accurately the proper rates of expansion for different steam-pressures, and it is probable that no fixed rule could be framed to include the modifications due to all conditions. We give the following provisional rule with tabulated examples.

"(6. E.) *Rule.*—To the number representing the steam-pressure above the atmosphere (P) add 37; divide the sum by 22; the quotient will represent, approximately, the proper ratio of expansion (R) for that steam-pressure. That is

$$R = \frac{P + 37}{22}$$

Examples.

Steam-pressure above the atmosphere	= P	5	10	25	40	60	80	100
Ratio of expansion	= R	1.9	2.1	2.8	3.5	4.4	5.3	6.2

"It is probable that these ratios are nearly correct for single engines of large size, with details of good design, too large for single engines of ordinary construction and too small for the better class of compound engines. The rule, though provisional, is safer to follow than the uncertainties of personal opinion and the variations of actual practice. Further information cannot vary it materially, for the economy changes very little for expansions considerably greater or less than the most economical grade. The limits of expansion for the higher pressures are apparently well defined by the experiments discussed, but there are indications that there is no loss in using somewhat higher expansions than given by rule for steam-pressures of 35 to 40 pounds,—of course, however, with results inferior to those obtained by using higher pressures. Further investigations are being made on this subject."

some experiments with a vessel at one time, I found that the engine had very much less efficiency than another engine of the same type, and about the same size. Thinking it over at night I found the cause. They had been regulating the draft by opening a damper in the connection doors, consequently cold air passed into the steam-chimney and prevented it from being anything like a superheater. When this was remedied the efficiency of the engine increased at once. It is the province of any one trying experiments to report all the facts; and any one reporting those two experiments together would either have to explain the discrepancy—which there might not be time to do—or else he would be sending data down for other people to figure on the two sides of the question; one would take one experiment for one side and another would be taken for the other side. The same thing has been done again and again with the experiments made by Mr. Isherwood. Mr. Isherwood in his experiments always insisted on running a very long time (there being usually some change of condition that varied the results), and averaging all these variations. I have found, however, that if one will look after every detail himself with great care and see that no error creeps in, he can make the experiments as a general thing compare together, though there will be times when a stray experiment will come in that cannot be explained. I state this preliminary because in the Novelty Iron Works experiment there were numbers of experiments which were difficult of explanation. The apparatus was designed before any naval engineers got there, and it had to be modified and changed a great deal. It was proposed at first to try the benefit of expansion by using the same quantity of steam in cylinders of different sizes to see how much work could be obtained. Now instead of sticking to that, so that there was no other question to be settled but that one question, the gentleman who designed the work put on a different style of valve-gear, a different style of piston, a different proportion of piston-rod, a different proportion of parts, and all sorts of differences in each of those cylinders; so that when the experiments came to be made it was a test of half a dozen things, or more, and not of one thing. Now the experiments will be found to agree very well among themselves which were made with the same engine to try the variation of economy in that particular engine, and two out of five or six different cylinders put into the engine compare together because they had the same styles of valve-gear.

But if the results from other cylinders be compared together wide differences will appear, and no opportunity be found for drawing any conclusions that are legitimate or that appear to be bound by any rules. And this brings me back to correcting a suggestion I made awhile ago, the same difficulty would be met in collating experiments made by Tom, Dick, and Harry all over the country and all over the world. There would be such differences in the construction of the engines that they could not properly be compared one with the other. Only when you take the same style of engines would the comparisons be of any value. The Corliss engines, for instance, all over the world would compare well together, but differences would be found even in them. The matter must finally be fixed on the general basis of taking in all the conditions.

MR. LEAVITT: Mr. President, I do not propose to consider this subject at all in a theoretical way. My first remark is that I hope all my competitors will adopt the formula of Messrs. Wolff and Denton. If the Corliss engine at Pawtucket will run the most economically, all things considered, at 4.84 expansion, I think Mr. Corliss would have found it out, for he made innumerable experiments on that engine; and the absurdity of this statement is shown in the fact that the pumping plant would not be changed by any change in the size of the cylinder. All the variation would be simply in the cost of the cylinders, which would be a mere bagatelle. The type of engine would have to be changed entirely in order to realize any gain from a reduction of expansion. In the first place it would be absolutely impossible, with the initial pressure adopted by Mr. Corliss, to run the Pawtucket engine at 4.84 expansion without wrecking it. I have made experiments with pumping engines having different ratios of cylinders. I have tried the small engine as suggested by Mr. Emery to do the same amount of work under the same conditions, the small engine having the advantage of a higher piston-speed and a higher initial pressure of steam—115 pounds of steam—and a large engine running with the steam admitted to the cylinder through a reducing valve, so that the initial pressure in the cylinder was 75 pounds instead of 115, and we saved enough in fuel in a year to pay a good interest on the whole cost of the plant, the ratio of expansion in one case being double that in the other. My experience in a large number of experiments with engines of different types and different initial pressures, corresponds very closely, as far as the efficiency of

the fluid is concerned, with the table presented by the President. The subject of initial condensation is one that I do not think we know very much about; I am sure I do not; but it is a very serious one, and I would like to illustrate it in regard to different ratios of expansion with the same cylinders. In the two pumping engines that I have tried to describe, the smaller engine makes a high

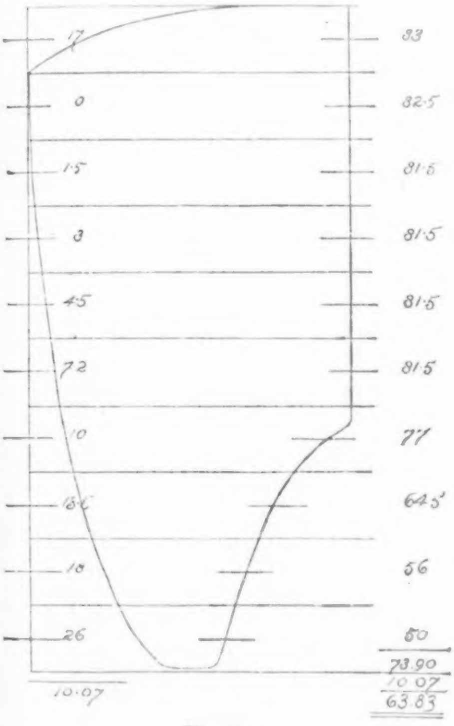


Fig. 59.

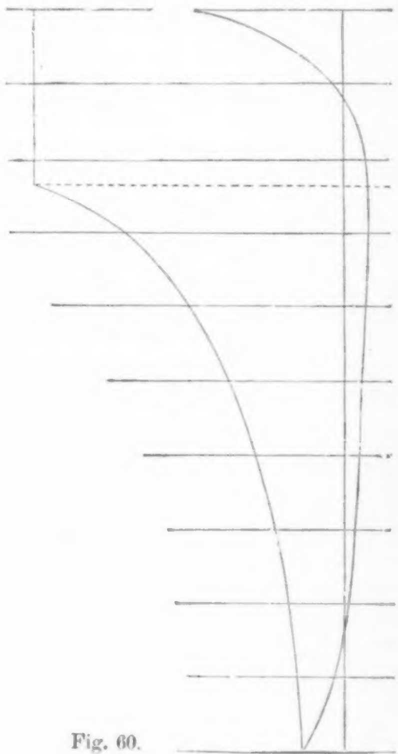


Fig. 60.

pressure card (*see cuts*), following very nearly five-eighths the stroke; the ratio of the cylinders being 4½ to 1. You will notice that there is a very serious fall of pressure at the end of the stroke. The clearances are very small—I think about two and a half per cent.; and taking the volume of both clearances (the high and the low pressure), the fall of the pressure to change the volume ought to come about there (*indicating*). In the large pumping engine there is a considerable wire-drawing of steam, and then the steam expands down, and makes a card about like that submitted. The economy of this engine, I have said, is something like twenty per cent.

over that, with all the favorable conditions of the latter. The construction of the one is as good as the other; and the only place in which I can locate the loss is in that point (*indicating*). The ratio of length to diameter of cylinders indicates that the small engine is much more favorable for efficiency of the jackets than the large engine; but, of course, conditions alter cases. We have lately made an experiment at the Calumet, with a high speed engine in which the jackets had been thrown out of use, owing to the fact that the low pressure cylinder was cracked; and I wish to tell my friends here who propose to build compound engines that I have found that it won't do to cast the jacket solid. It will run for a while, but it will sooner or later crack. This engine ran for probably three years before we noticed any crack. The speed of this small engine is 325 feet per minute; the speed of the larger one is 240 a minute; and the speed of the engine with cracked cylinder is 480 a minute. The pressure of steam with the fast running engine is 126 pounds, and the cylinders are 18″ and 36″ in diameter, by 5 feet stroke, and it is driving a load of 600 horse-power, which varies from 450 to 640 horse-power. That engine, on an experiment extending from the 21st of December to the 1st of January, ran with $16\frac{3}{10}$ pounds of feed water per horse-power per hour. The thermometer ranged from 30° to 14° Fahr. the larger part of the time, and the outer wall of the low pressure cylinder was uncovered. Now here is something for us to think about. Those two engines were in the very best possible condition as regards clothing of cylinders, but something is amiss that we have this great terminal pressure from the high pressure cylinder, and in the other too small an initial pressure for the best advantage. It would appear that those two engines should do very much better than the other; but, as a matter of fact, the best engine there does about as well as the one here (*indicating*) without jackets. The Lawrence engine ran with 16 pounds and a fraction, very nearly the same as this, and the trouble in the Lawrence engine is also to be found in this same fall of terminal pressure in the high pressure cylinder. The expansion there is 18 times, and it comes down oftentimes to about that point (*indicating*).

I do not want Mr. Wolff to think I am making a harsh criticism on him; but the main thing in pumping engines is the pumps, and the size of the cylinders does not very much increase the cost of the engine. It is only a question then of the efficiency of the fluid. I find that all the working parts of the engine have got to be the

same practically within reasonable limits, whatever may be the size of the cylinders. In mill-engineering no doubt other questions come in. If we are able to leave off the jackets, as would appear to be quite probable from the experiments made, why the construction of compound engines is going to be a very simple thing; but very many other things have got to be taken into consideration in regard to that. It does not do to send the steam out from the high pressure cylinder at too high a pressure; my opinion is, from observation, that the cut-off should take place from a third upwards; that is, in the compound engine the cut-off wants to be operated near what it would be in a single cylinder engine of the same power; because there is a trouble in getting rid of the exhaust, and then there is danger of bringing too great a strain upon the parts.

Mr. Wolff: I would like to offer one or two remarks. First, I would like to impress upon Mr. Leavitt that I am very much obliged to him for calling attention to the requisites of pumping engines. But I would like to call his attention to just one fact, that the interest on the cost of engines is not the only element which enters. "Cost of engine" does not mean alone interest on the cost of engines, but it means also the wages of the engineer force; it means the depreciation in value of engine, cost of oil, etc. As far as he comments on the Corliss engine—that it would break up if run at that expansion—of course we cannot take that fact into consideration in our analysis. It may be that the most economical point of cut-off to be used in that engine is not one which is practicable, and that it might not prove the most economical if the engine breaks. But if properly designed, the engine should not fail. But I desire to have Mr. Leavitt understand particularly that I do not consider at all that he has been harsh on me. It affords me great pleasure to be able to call out the opinion of a gentleman for whom I have so very much respect as I have for Mr. Leavitt.

Mr. Stirling: About the wages of the engineers. I fail to connect the wages of the engineer with the point of cut-off in the cylinder. I do not see what influence that can have at all in determining the point of cut-off. I have had occasion lately to change some engines of the ordinary non-condensing type and without cut-offs, to make them cut-off engines, with condensers, and I find that the engineer opens his throttle and starts his engine in the usual way, and the cut-off and condenser do not give him any additional trouble. I do not think that there is any reason for the

engineer's wages to be increased under these circumstances, and why we should introduce this element into a formula for determining the point of cut-off I fail to see.

MR. WOLFF: I would like to call attention to the fact that I do not think, in the case Mr. Stirling has referred to, the wages of the engineer would affect the problem, that is to say, the wages of the engineer would not cause the economical point of cut-off to change. The wages of the engineer enters as a constant quantity to affect the ratio of expansion, and I think we have gone so much in detail into this question in our paper that I cannot do better than refer Mr. Stirling to the same. We were fully aware when we prepared this paper, that a number of objections would be made to it which would be met in the paper itself, but which would not be properly appreciated until it was read.

MR. DURFEE: I would like to submit for the consideration of the authors of the last paper read, that if some enthusiastic engineer, who was not particularly well posted in the use of formulæ, should apply those of the authors of the paper to determine the expansion proper in any particular case, and that a smash-up should result, he would wish that he had used empirical formulæ, or some of a different character from those he had experimented with.

MR. WOLFF: Mr. President, I fail to see that there is any connection between that point and our problem at all. The question of economy is not a question of smash-up, and the question of smash-up does not determine the question of economy. I fail to see the slightest connection between the two. A man might, for instance, find that a certain part of his engine would stand a certain strain. If he should then put a strain on the part beyond that, he would certainly be responsible. And so if a person found that the most economical point of cut-off would cause a smash-up, or if his experience would lead him to believe that it would cause a smash-up, I should think he would sacrifice the question of economy. Then there is another element that enters this question. This formula is not alone to be used in engines that are designed, but it is to be used when engines are to be designed. Now if we find the most economical point of cut-off is such that it will probably give a smash-up, we shall probably make up our minds that we have not adopted the right style of engine, or right proportions of cylinder and parts of the engine, and we will start again and adopt some new dimensions of cylinder and engine, or of its parts

and work up our most economical point of cut-off until we find that we get such as will not give a smash-up.

Mr. Durfee: I entirely disagree with the statement of the last speaker. I think if these formulæ are worth anything at all they are worth using on the engines in use to-day. The gentleman tells us that an engine well known to be an economical one is run at altogether too high a rate of expansion—that an expansion of one-quarter is an economical rate. Now another gentleman, whose experience is very large, and who has the confidence probably of us all, comes and tells us that if that engine was run at the grade of expansion that this gentleman's formula gives an inevitable "smash-up" would result. Well, now, there are a great many engines running under precisely those conditions. There are a great many engines in the country to which it would be quite as dangerous to apply the formulæ of the last paper as the one at Pawtucket. Now what is the use of these formulæ or this paper if when people come to apply the doctrines inculcated they are going to tear to pieces four or five or ten thousand dollars' worth of property? What is the utility of it? I fail to see it.

Mr. Wolff: In answer to that I should like to ask Mr. Leavitt a question. I should like to ask for what cause a ratio of expansion of 5 in the Corliss Pawtucket engine would give us a smash-up?

Mr. Leavitt: Because you put so much pressure of steam on the low-pressure cylinder.

Mr. Wolff: Would it not be possible then to carry a lower pressure of steam?

Mr. Leavitt: Then you would defeat the economy. By reducing the pressure of steam you would sacrifice the advantage of high pressure.

Mr. Wolff: In case of using lower steam pressure we might make it up by not expanding as much.

Mr. Leavitt: I will answer you that by some experiments I made at Lynn. The contract for the Lynn engine required us to do the work with 45 pounds of steam. We built the engine to carry a higher pressure, intending to do it if the commissioners would let us. The boilers were also built for high pressure, and in order to satisfy them (the commissioners) that we could fulfil our stipulations we made a great number of experiments for a long time on 45 pounds of steam, and we used to get about 75,000,000 to 78,000,000 of duty. When the engine was tested by Mr. Hoad-

ley, Mr. Davis, and others, with 75 to 80 pounds of steam, they got 103,000,000; the expansion then was about 14; in the other case it was between 5 and 6.

Mr. Wolff: You understand, Mr. Leavitt, that as far as the efficiency of fluid is concerned we do not at all say that the formula that we present would give the smallest consumption of coal.

Mr. Leavitt: Exactly, but here is an engine that would not have cost a cent less if built to run with only 45 pounds of steam.

Mr. Wolff: Did you get the same amount of work?

Mr. Leavitt: Yes, more; the engine ran at the same speed. It had to deliver a certain number of gallons of water in twenty-four hours; all the conditions were the same, the only variation being the boiler pressure.

Mr. Wolff: Into the question of interest of cost in any particular engine enters the element of cost by itself. A change in the ratio of expansion, while it will at all times affect the interest of the cost of engine, may do so inappreciably for small charges; but the effect of this constant quantity always has a tendency to lower the ratio of expansion which efficiency of fluid would give. I feel confident that these things will not be made any clearer in hasty discussion by anything I have to say outside of what is said in the paper; so I feel for one that I should like to be silent, except when such points are brought up as would make that signify that I at all felt any lack of confidence in what I have presented, for I have the confidence that a man has who believes he is in the right. I have the assurance that when the paper is read and fully appreciated it will be seen that these different questions which have come up this evening have in the main been met with an answer.

Mr. Emery: Mr. Leavitt has given the size of cylinder and revolutions per minute. I would like to ask in relation to the larger engines what the horse-power was?

Mr. Leavitt: $17\frac{1}{2}'' \times 36''$ develops 115 horse-power, and the next one very nearly the same.

Mr. Emery: Can you state the expansion in the two cases?

Mr. Leavitt: The expansion in the large engine is between 17 and 18 and the expansion in the small engine is between 8 and 9, and the steam pressure is 115 in the small engine and reduced to about 75 in the case of the large engine.

Mr. Emery: From the same boilers?

Mr. Leavitt: From the same boilers.

Mr. Emery: There is one without jackets for this same duty.

MR. LEAVITT: These are 18″ and 36″ diameter by 5′ stroke, 48 revolutions, 116 pounds the initial pressure of steam.

MR. EMERY: And the expansion about how much?

MR. LEAVITT: The expansion there is a variable quantity, because the load is very variable, but I should think it would average about 10 to 12.

MR. EMERY: Now the horse-power, if you please?

MR. LEAVITT: The horse-power averages about 550; but there is something remarkable about that engine—the fall of pressure is very slight between the two cylinders; there is that constant drop at the end of the card, and yet the clearances are larger than in the large engine.

MR. EMERY: They are all supplied from the same boiler.

MR. LEAVITT: No, no; the engines are four miles apart. Of course I cannot say that the quality of the steam is not a great deal better for the high speed engine than for this. I have no doubt but what it is. The steam is taken from locomotive boilers, carrying 126 pounds, and this is taken from an elephant-boiler, where there is a very great water-level and a great quantity of heating surface. The steam-pipe is about three times as long as it is to either of these engines. These engines stand side by side so that the steam-pipe is the same length.

MR. EMERY: These are some practical facts from which to derive conclusions on the subject, which are worth a great many theories, just as I have stated; and it appears as though the small diameters would have some influence on the matter as well as the expansion. Both are jacketed, as I understand, the same?

MR. LEAVITT: Yes.

MR. STIRLING: I wish personally to express my thanks to Mr. Wolff for his paper. I think we should take these two papers which have been presented and combine them with our everyday practice.

MR. HARRISON: Mr. President, there is one point that has been brought up here which I would like to get light on; that is the use and non-use of the steam-jacket. I have read some accounts of experiments in England perhaps a year or two ago; and one experiment was cited in which the jacket-water was measured away for a period of one hour with the engine standing still, and the weight of water was very much the same as if the engine had been running. Do you know anything about that, Mr. Emery?

MR. EMERY: I do not; I have not made any experiments with

jackets in such a case. It has considerable influence under certain conditions. In the case that is mentioned here, I think it is possible that the speed of revolution was the cause of the difference, as was very clearly mentioned in the paper of the President. I think he called it "speed of piston;" if he will allow me to correct him I think it should be *speed of revolution*. The number of alternations per minute is an element to produce economy as well as the steam-jacket, and acts in somewhat the same way. The steam-jacket by keeping the temperature up prevents initial condensation, and therefore, the steam being dried, there is not so much heat absorbed, as the steam leaves the cylinder at the end of the stroke. Now high speed acts in something the same way. The metal is not heated or cooled to so great a depth. There is not so much material acted upon, and therefore there is not so great a loss. If we increase the speed we decrease the efficiency of the jacket, because the jacket depends for its efficiency on slow speed; and when we increase the speed the jacket has less efficiency, but we gain in other directions.

MR. LEAVITT: I made a mistake, which I wish to correct. We have changed one of the cylinders on that engine, so that the high pressure is $22\frac{3}{4}''$ instead of $18''$ in diameter. We made experiments with that same engine when it had $18''$ and $36''$ cylinders and was driving a load of 200 horse-power, for which the consumption of feed-water per horse-power per hour was 23 pounds, but the boilers were different, and so there is a question that comes in.

MR. EMERY: The range of the temperature is such that we have to use a larger cylinder at first in order to get the temperature low enough to divide the temperature between the two cylinders. That is the reason for my practice of using four-tenths instead of one-fourth.

MR. LEAVITT: I would like to say a word in regard to the consideration that engineers are obliged to give to the commercial aspects of the case, and I can best do it by narrating a fact. Recently the Calumet and Hecla Mining Company wanted an engine; and we offered the Worthington people two thousand dollars bonus to build that engine if they would build it by a certain date. They could not do it, and so I was obliged—and very reluctantly—to get up a design and have an engine built. But it is not the best thing for us commercially; it is only because we are driven to it. I had the option to do as I chose entirely. But my reputation as an engineer would not allow me to put in a more costly machine

when the advantages would not save a handsome interest on the extra cost.

Mr. Durfee: I want to say a few words in regard to the discussion of last evening. There is nothing more difficult than a correct observation of facts, unless it may be a correct inference from the facts observed. I remember an illustration of this remark. Some years ago an enterprising inventor attempted to introduce a patented plan of "setting" for steam boilers; and in order to demonstrate its advantages some experiments were tried with a certain kind of boiler—in fact two boilers of the same kind were used—one of which was set with the ordinary arrangement of brick-work, and the steam generated was worked off through an engine. The second boiler was set with the patented setting, and the steam when it arrived at twenty pounds pressure was blown off through the safety-valve. The pressure in both boilers was kept the same. After a great deal of weighing coal and taking temperature of flues and weighing water, etc., the experimenters arrived at the conclusion that there was forty-five per cent. in favor of this patent setting. Some doubt was expressed as to the correctness of the investigation. The whole thing was gone over again, and the same result within two or three per cent. arrived at. On the evidence of the aforesaid experiments parties who thought them conclusive proof of the value of the patent had it applied to their boilers, and to their great astonishment, after some few months' experimenting, they found that their coal pile shrank about as fast as with the old arrangement. Then it occurred to somebody that the conditions under which those experiments were tried were not the proper ones, and when they connected the boiler having the patent setting (in the original experiment again repeated), with an engine, and worked the steam off quietly, it was found that the patented devices were no improvement over the old and well-known methods. The experiments first named were very elaborate; the reports on them were quite as elaborate as the experiments, and the results were not worth the powder requisite to blow them up, and it would not have needed much. The experiments of which I have made brief mention, though in the first instance planned by men of scientific reputation, and conducted according to their advice in every particular, were not only utterly worthless, but what was worse than worthless, they were misleading. Had a clear-headed experience been called originally to the consideration of the problem, a large saving of expensive disappointment would

have resulted. Now, to apply my illustration to the matter under discussion, and more particularly to that of the last paper read, which (though I cannot agree with its conclusions) challenges my admiration as an evidence of great labor and painstaking perseverance.

When an engineer departs in his practice from well-known and accepted methods, it is not only customary, but right, for the public at large, and more especially his employers and associates, to demand a reason for the "faith that is in him," and an explanation of the why and wherefore of his action; and whenever such a departure from well-established practice is made, in steam and kindred machinery, on whose strength and proper behavior in operation the safety of many lives depends, it is of more than ordinary importance that such departure, whatever it may be, whether in the strength of parts, their relative arrangement, the action of the steam or other impelling force upon the machine as a whole; or last, but not least, in the formulæ which determine and define the relation of all of these to each other, should be examined with critical particularity and its value determined with judicial impartiality.

In the case of the paper last read we have a most decided departure from accepted practice, a departure, sir, not only radical in theory, but if possible still more radical in the practical application of that theory to the daily working of a most important class of machinery.

By this new revelation which we are invited to accept as a "lamp to our feet and a guide to our path," we are asked to say that the ablest experts and practitioners known to our profession are mistaken. We are asked to admit conclusions, which, if admitted, prove every engine afloat on ocean's broad expanse to have been conceived in ignorance and constructed by stupidity. We are told that engines which are celebrated for their almost phenomenal economy would be still more economical if, instead of expanding their steam seventeen times, their owners would but change their "cut-off" so that the steam would be expanded but four times!

Now, sir, I firmly believe that the persevering authors of the paper in question have, like the original experimenters with the boilers named in the outset of my remarks, omitted some essential factor, or misinterpreted some important fact; and as an inevitable result the formulæ which they offer as exponents of their theory,

though costly in painstaking perseverance, are of little value; in fact, I am constrained to believe that what is here proposed as remedies for all the ills that the steam-engine is heir to could be boiled down into some such form as this:

$$x + y + z + \& - a^2 \textit{ non plus} = 0.$$

In this expression, x, y, and z, represent the three general directions in which steam or any similar elastic agent may exert its force, and the ampersand, the value of the fourth form of extension, which some abstruse mathematicians have been bothering their heads about; and while the quantity a^2 *non plus* seems to have a negative value as it stands, it will be found by transposition to have a sufficiently positive significance.

Mr. Hutton: There seems to me to be a point deserving of note in this discussion, coming up incidentally from a remark made by Mr. Emery in the discussion last evening. In reference to the advantage from an economical standpoint, with regard to the efficiency of fluid, he spoke of the use of a high number of revolutions. That involves, of course, when we take the old formula, the horse-power equal to

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That, given a number of horse-power for the engine to develop, if we increase the number of revolutions, we will probably reduce the length of the stroke; and with a given expansion in a short-stroke engine, it will at once be seen that the necessity for the development of a certain horse-power would require a greater initial pressure at the beginning, in order that we may develop the horse-power in this short stroke. The area of the cylinder being consequently enlarged, we shall get a large initial pressure when the steam is admitted from the boiler. Now any who have ever had any experience on engines with ratio of a stroke to diameter as 1 to 2, as compared with the working of an engine with a ratio of say 5 to 6, must have noticed how much harder the engine apparently labors when we have got a nearly square cylinder than it does when the cylinder is long in proportion to its diameter. By the strain on the brasses and on other parts of the engine the engineer can feel that his engine, if it has a square cylinder, is laboring to a great extent. It is apparently tugging at the crank-pin, and not infrequently it is almost instinctive, with an engine of adjustable cut-off, when the engine is felt to be

laboring in this way, for the engineer to throttle off a little, reduce the initial pressure and enlarge the admission. The effect of that, of course, when we get such a condition, is that we have the short stroke involving a heavier weight of fly-wheel, which, when the engine is not proportioned on the Porter system, throws an extra amount of strain on the crank-pin during the second half of the stroke. It seems to me that we combine here with the question of economy of fluid—the economy of fluid resulting from the short stroke, and consequently high number of revolutions—the fact that the latter produces a strain upon the working parts which must necessarily wear that engine out when not designed according to the best rules of practice to-day; so that it seems that these two things are here directly opposed to each other, the economy of fluid and the economy of repairs, and possibly incidentally the wages we would have to pay to the engineer in order to make these repairs.

Professor Thurston: (Mr. Leavitt, in the chair.) In closing the debate on these two papers I have very little to say. In regard to the general character of this problem members have seen, and it was shown very plainly by the character of the two papers presented last night, that there is a problem within a problem. The first of these two papers attacked one of these problems, a subsidiary problem; while the second attacked the more general and, in some sense, the more important problem. In order to determine the amount of steam and coal to be used in any given case, it is necessary to know what will be the behavior of the steam in the engine. In order to know what is to be the behavior of the steam in the engine, we must know what are the conditions under which it is to be used. In order to know the conditions under which it is to be used we have to study the pressure of the steam used, the character of the steam supplied to the engine, the way in which that steam is treated after it reaches the engine; and this last point includes the determination of the way in which heat is transferred to and from the steam while it is doing its work in the engine. As shown plainly in the first of these papers, the expansion-line that we get in the steam-engine is neither isothermal expansion nor on the adiabatic curve. It is not either of the two curves representing the expansion of the steam in the second paper. It is a curve that belongs to no class. It has been assumed that the formula of expansion may be represented by a curve of the hyperbolic class. I doubt if the

exponent ever becomes the same in any two engines; and even in the same engines, at different times of the day and under varying conditions of the day, I doubt if the exponent remains at all the same. And the curve is one that is determined not only by the fact that you cut off steam at a certain point, and have a certain kind of fluid there, but by the fact that this fluid is affected by interchanging heat with the surrounding surface. I merely call attention to the point, because it is shown plainly in the paper; I bring it up here simply to make use of it presently. So that the method of expansion in the cylinder must be studied before we can say how much steam we can use; then, in addition to the amount of steam in the cylinder, we find we must supply a certain amount of heat to make up for the loss that has accrued from the transfer of heat that does not do work. So that the study of a steam-engine as we find it treated ordinarily includes only a part of the problem. The method of treatment that is now standard is one that involves only the thermo-dynamics of the steam-engine, and it is assumed by the most prominent of writers that the expenditure of heat is simply that required to supply a demand for conversion into work and a certain loss that occurs inevitably when expansion has become complete, and this heat which then remains which has not been utilized must be thrown away. That follows from natural conditions that are beyond our control apparently. And those two directions of the expenditure of heat are the two that are considered usually in treatises on the steam-engine. As soon as the engineer begins to study the actual facts of the case, however, he finds that this accounts only for a part of the heat that he expends, and in a great many cases it is but a small part. In very many cases, probably in the majority of cases as engines are usually constructed and operated, the amount of heat that is lost by other methods of expenditure is greater than the amount that is expended by conversion into work. The paper that was first presented simply embodied an attempt to show what were those amounts, and what would probably be the expenditure in such cases as occur in actual practice. Now no investigation, as was stated in that paper and in the second paper, has yet been made that enables us to say in any given case what will be the amount of heat lost by these other methods of expenditure. And consequently all that I have been able to do, and all that any man can to-day do, is to study the work of the engine as he finds it doing its work, and, by comparing actual cases,

construct empirical statements of the probable amount of heat so expended.

Now adding to the amount of heat converted into work, the sum of the other two amounts, the necessary waste, by rejection of heat into the condenser, we have the total expenditure of heat; and this last amount being known, the expenditure of heat can be predicated as accurately as the velocity with which a heavy body will fall through the air to the ground from any considerable height. So that that first problem necessarily precedes the second, and the character of the problem, I think, is sufficiently well stated in the paper to require no further amplification here. The method is adopted simply perforce; simply because we can do no better. The results that are obtained are as accurate as can possibly be obtained without a long extended and careful investigation that shall develop the influence of all those points that were catalogued as bearing upon the expenditure of heat, and consequently the engineer can to-day only take this method. I see no other way—I presume no engineer can see any other way—of determining how much heat will be expended in any given case. An engineer has to design an engine which will work with the greatest efficiency under those conditions, and he must be able to say about what will be the expenditure of heat and steam; and he must, if he is doing business as we ordinarily do business now, be able to guarantee to the purchaser that it shall do its work at an expenditure of so many pounds of coal, or so many pounds of steam, according as he supplies the boilers or not. Neither Mr. Leavitt nor Mr. Emery, nor any other man designing engines, can say exactly what is to be the expenditure; but from his experience he knows about what it is to be, and by some formulæ or empirical method he determines a figure upon which he is willing to base a guaranty, and upon which he will, to a certain extent, risk his reputation. We are driven to this thing perforce; we cannot avoid it. We must, if we are to do our business as we ought, find some way of getting a probable result. The attempt in the first paper was to determine what would be the best probable results under the best probable conditions, and, so far as I can see, and so far as the engineers with whom I have talked can see, those results are fairly represented by the figures that were tabulated. In some cases you will find, as Mr. Emery remarked, a better result is obtained; in a great many cases a worse result is obtained; the figures are not given as the best possible results. In the opening of the second paper reference is made to a class of

formulæ that has been proposed to cover the total expenditure in determining efficiency, and, as they are all empirical, the writer of the paper says justly that they are objectionable: but it would be vastly more objectionable to have nothing; and so far as I can see, so far as engineers whom I know can see, we are driven to their use. And again, in referring to that class of formulæ,—formulæ addressed to that object,—the writer of that paper states that they are objectionable in form, objectionable in kind, and that they are not correct in result. It is true they are not accurately correct for all cases; they are not rigidly correct for any one class of cases. But the remark that I have already made still holds good that we must use them, from the fact that we have nothing better; and the duty of a man who criticises the empirical formulæ that have been presented is not only to show in what they are defective, but to show how they can be improved, and to give us improved formulæ; and the man who is not able to give us anything better should not find any fault with the formulæ as he finds them. That remark I make with some deference.

Now the second paper attacks an entirely different problem. It is to determine whether it is worth while, under certain conditions, to put up an engine and go to the expense of getting high efficiency. The first is a purely scientific question; the other is a purely commercial question. The first is a question of purely constructive engineering; the second is a question that is purely commercial, and assumes that the other problems have been already solved; that is, that it can be determined what is the cost of an engine expanding at various ratios of expansion, and what is the cost of coal-consumption; what is to be the cost of steam consumed. It assumes that it can be known with exactness what is to be the interest on the cost of plant; that it shall be known with exactness what is to be the cost of wages to the engineer and attendants, and what are to be all the costs that the treasurer of the concern will be compelled to meet when he puts in one or another of these several engines. It assumes that all of those points are definitely known, and can be so put upon paper as to enable you to reach a certain result that will be exact and definite. All that is exactly true, and I think the writer of the paper does not at all undertake to meet all the problems or all the methods that he has presented. At the last meeting I think some of the members will remember that I called attention generally to that direction of investigation. I remember making the remark that on our railroads it will be found

that about one-fourth of the average cost of running a locomotive engine was the cost of fuel; the remaining three-fourths included wages, repairs, etc. And that statement did not include interest on the first cost of the machine. But an engine running as we find them running on our roads expends, we will say, 17 cents per running mile, for running expenses. Of that 17 cents $3\frac{1}{2}$ to 4 cents only are for fuel. So that, as I remarked then, the directions in which we are to look for economy are, in some cases, quite other than the directions in which we are to save steam and coal; that these other economies may assume vastly greater importance—or some greater importance, I will say—than the simple cost of coal. Now the second paper attacks this very problem, and attempts to show that we have an exact method of determining exact results. As to the method, I do not see that there is any room for our criticism. It seemed to me so important a matter when my attention was called to it, when I began to realize how great were these other items, that it looked to me as though I had wasted my time in preparing that first paper, and I was very strongly inclined to throw it up, and say this is all nonsense; the investigation is hardly worth making, and whether the results obtained are obtained by exact formulæ or obtained by empirical formulæ is a matter of not the slightest consequence. Well, thinking it over, I thought I would at least let the matter stand. There would seem to be room for doubt as to whether it might not be a matter of importance to determine what might be the consumption of steam and cost of coal, and what, the conditions being given, would be the probable cost in fuel and in steam. So the paper stands. But I think that all engineers, who have had any extended experience, have been compelled to recognize the importance of this second problem, and, although they have not adopted the exact method shown in that paper, they have been compelled by some process, probably empirical, to get at results that are approximately and fairly, and sometimes satisfactorily, correct. I doubt if any engineer, in the trade of building engines, would attempt to build a compound engine with an elaborate expansion-gear to send down among the coal-mines of Pennsylvania to hoist coal. So far, I think, the experience of engineers has been a tolerably correct guide to them; but, so far, they have not had an exact method of determining an exact figure. I do not think engineers have recognized the fact that the ratio of expansion in an engine is an element to be considered in relation to all these items of cost. Therefore I should say that

the subject of this second paper has immense importance and that the paper is very valuable.

In the paper, the writer has not given the exact weight that should be given to many of these items, and therefore he has not given as a result the very best ratio of expansion, all things considered; but that is a matter of minor importance. A matter of greater importance is that here is indicated a method of getting results in which, if properly carried out,—if the conditions are stated so as to be known accurately,—the result will be accurate. And when you say five expansions, you mean exactly five expansions; and if you find it necessary to come down to three expansions, for financial reasons, it *is* three expansions; and you have not the liberty of going from three to five, saying it does not make much difference which. Now in the matter of efficiency of fluid you will find a different state of things. You will find that it will not make much difference, under a certain set of conditions, whether you expand three or five or twelve or thirteen times. But when these other controlling considerations are brought in, in the way in which they are brought in in the second paper, they do enable you to get results that, in so far as primary conditions have been made exact, are themselves exact. If your conditions have been studied so as to be stated accurately, I think the result will come out accurately; and I think the paper has the very greatest value as pointing out the way, and I am not inclined to criticise results very closely. I have not the slightest doubt that if the authors of those papers were to study special cases as closely as designing engineers are compelled to study them, they would get results that would run wonderfully close to the proper mean. Now there are some inaccuracies that come into that paper as it stands that seem to me important as affecting the results attained, not as affecting the value of the method at all. The work outlined in the paper is carried out by the study of certain curves. Those curves are laid down in the lithograph handed you last night. You will find one is a curve based on the assumption of hyperbolic expansion, and the other on adiabatic expansion. The difference of those curves was shown to be sufficient to make a difference in the rate of expansion. The character of that curve seemed to me, as I followed the debate,—I may be incorrect,—the character of those curves determines to a very considerable extent how accurately your results shall be obtained. Now it is the fact that in the steam-engine you never get hyperbolic expansion. Never in the history of the world

was hyperbolic expansion obtained in the steam-engine, and I presume it never will be obtained. It also is a fact that in no steam-engine ever constructed do you get adiabatic expansion. You get one of these peculiar curves that no man has yet traced with perfect accuracy, and consequently the mean pressure will vary to such extent that for every engine the curve will be different from every curve that you will have laid down in constructing other engines and in studying other conditions affecting the same engine. Now, in order that this result shall be rigidly accurate, all the points that were indicated by the writers of the paper must be indicated with perfect accuracy. Now (*illustrating*) there is the hyperbolic curve as laid down. There is the adiabatic curve or the curve based on the adiabatic curve. From the same point on the base-line we draw a line on a tangent to one of these curves. Suppose now this correct curve has some different form. Assume that it starts more like that and takes that form (*illustrating*). You will find now that the point of cut-off—assuming this to be the best point from which we should start off here somewhere (*indicating*)—that the ratio of expansion and the point at which we cut off will be very much more affected by the differences that involve the position of this point than on either of those two curves. On the other hand, suppose we have a curve like that (*illustrating*). Then you will find that the ratios of expansion are affected almost not at all. You are going to cut off at about one-half, we will say, or one-third. The point would come, we will say, at more than one-half. Going that way we get lower ratios of expansion, and it may come down if you cut off at three-fourths; and it will vibrate about that point with very little variation, no matter what conditions you are allowed to interpose by commercial considerations. So that I think I can see plainly that the form of that curve is one of the essential elements in securing accurate results. Now the determination of the form of that curve is one of the very things I have been talking about as being so very difficult of attainment. Yet I am not at all certain that if that curve is found, and variations from that curve are determined, but that, it may be, the curve will take a form approximating to those already laid down, and then the variations of expansion will be very slight for very wide ranges of efficiency of machinery. But if there is a fallacy—if there is an apparent fallacy—it will probably be found to be based upon the assumption of the form of these two curves. It is a point, I suppose, the authors will look up, and finally I suppose they or some-

body else will give us a means of making a very exact determination of the proper point of cut-off—the proper ratio of expansion for an engine going into any given place; and they can go to the treasurer of a cotton-mill, or the proprietor of a coal-mine, or anybody else, and say "for your purposes the best thing to do is to put in an engine having a ratio of expansion of three or four or five times, as the case may be, and that will be the exact figure." And, putting that engine in, it will be found that when the accounts are made up at the end of the year the engine will be doing better than any of the neighboring engines that have not been so adjusted.

Finally the authors of the paper animadvert very strongly upon the empirical formulæ. What is said there is, I think, just—that the man who presents an empirical formula takes a certain responsibility. He presents something that is erroneous. It may be nearly correct, but it is not rigidly so. He induces people to say this is a hard and fast line, and to follow that line, although it is not the correct one. So long as this empirical formula does not exist people who are groping about may hit the right thing. The use of an empirical formula does involve a wrong principle in itself, and yet *the work of the world is done upon empirical formulæ*. We cannot do without them. We are using them to-day, and we have used them from the beginning; and the best work done in engineering has been done upon the basis of empirical formulæ.

The second paper presented is to a large extent based upon empirical matter. The theoretical work that appears in our treatises is carried exactly to a certain point; then it is dropped, and results are obtained by methods that to a certain extent are empirical; and it is true, and will remain so probably to the end of time, that engineers will do their work very largely, if not principally, upon empirical formulæ. In illustration—we all accept Regnault's determination of the pressure of steam at various temperatures. The table that we use is Regnault's table, based upon a series of experiments. It has not been deduced theoretically and is purely empirical. In that consists its great merit. When we use Regnault's curve, we know that we have—and that with all the exactness that any man can work to—statements of exactly the pressure that you are to have when you get a temperature of 320 degrees in the steam-boiler; you will find that figure, about 75 pounds, is to all intents and purposes exact. The curves that he obtained represent exactly the series of pressures that he obtained, and represent exactly the law of variation of pressure with temper-

ature. When he had made his graphic representation of this series of temperatures and pressures, he constructed a formula which he made to follow as closely as possible the line that he had drawn. Regnault's formula was for many years taken to represent exactly pressures and temperatures, and they are used to-day by many engineers; and probably the majority will take Regnault's with some other set of formulæ, which are simply empirical, and deduce what is expected to be the temperature of steam at a certain pressure and use that. Now, some years later Professor Rankine proposed a formula which follows Regnault's results so accurately that no man can see the difference; and Rankine's formula, which ties temperatures and pressures of steam together, is to all intents and purposes an exact formula, but is purely empirical. The formula was made up by beginning with a hypothesis which on the face of it was not correct, but which for his purpose was correct enough; and the formula which he hit upon, basing his work on this assumption, proved to be so rigidly exact that it can be applied to Regnault's results; and his formula carries you through the whole range with such accuracy that no man can see the difference by any measurements with instruments. Again, study Rankine's "Steam-Engine," and you will find that in that treatise on the steam-engine he brings out the relation of mechanical to thermal energy. He applies the laws of thermo-dynamics to the determination of the probable expenditure of steam in the working of his engine; and finally he gives you a set of tables in which he says an engine carrying a certain amount of steam cutting off at a certain point, and having boilers of a certain efficiency, will expend a certain amount of coal per hour. At the beginning he starts with exact formulæ, and after he has made his exact formulæ he says these are too cumbersome for exact work and we will take empirical formulæ. And then he says that you can have a formula that exhibits the relations of pressures of the volumes of steam to the hyperbolic form. The formula is not exactly correct, and the index is not exactly correct. The index is widely incorrect, but the results of the use of that formula are practically correct, and Rankine had sense enough to see that these empirical formulæ were very much better for the engineer's work than exact formulæ. But the engineer has the privilege, at the end of his work, if he wishes greater accuracy, of going to Rankine's exact formulæ and getting at results that are accurate. Now those results are accurate just so far as they represent the conversion of

heat into work, and no further. So when you come to the end of Rankine's work you find results given that no man ever sees. Not an engineer in this world has ever been able to obtain what Rankine gives as the results predicted by his method of analysis. So, assuming that they had been obtained by an exact formula, you find when you come to the end that they are widely incorrect, not because they are not exact, but because they do not include all the conditions. Including all the conditions, the engineer finds he gets results that Rankine does not; and no man will ever find engines working practically under the conditions taken by Rankine, and get the results put down in those tables. With small measures of expansion the results coincide pretty closely. With the old Cornish pumping-engine Rankine's results come out remarkably close; in fact they cross the line and are false on the wrong side. But with high measures of expansion, and without special provision against these other losses, you get results that are not Rankine's results; and the fact that the results are not exact as shown there, follows from the other fact that in this study of the steam-engine no account is taken of the character of the engine as a machine, and with the same conditions, so far as they are stated by Rankine, you may get widely different results, and if the one accords with Rankine, the other must be wrong.

So it simply brings me right back to the point from which I started, that the use of empirical formulæ is necessary to the engineer. He finds almost no exact formulæ that give him exact results. The formulæ themselves may be exact, but they do not fulfil all the conditions. Bringing in all the conditions, as is necessarily the case in actual practice, he finds the results do not accord with an analysis, exact so far as it goes. So I do not see any probability of our being able to dispense with empirical formulæ, and I do not see anything in any argument presented yet to justify the engineer in throwing aside empirical formulæ. All he can do is to do just what Rankine did, make empirical formulæ as exact as may be, yet they are always empirical. So he guides himself by experience—empirical formulæ representing experience. Now experience is changing; and as the engineer goes on with his work and improvements are made, it is found that he must change these formulæ. Experience being different, the formulæ that represents experience must be changed; and the intelligent man who keeps a little in advance of his time, and makes use of empirical formulæ, represents all the latest and best experience.

XXXVI.

THE BINARY ABSORPTION SYSTEM OF ICE-MACHINERY.

BY H. F. J. PORTER, M.E., NEW YORK CITY.

To hold complete sway over the elements will, I am safe in saying, never lie within the province of mankind, but that we may practically approach this result by governing their effects upon our comfort and causing, by mechanical appliances, their complete subservience to our ends in certain of the industrial arts, is to-day a certainty.

The means for accomplishing these results, science is daily augmenting, and as the demand has increased, so far the engineer, the chemist, and the metallurgist have been able, with more or less success, to grant a supply.

We have only to point to the civilization of to-day as the result of our ability to attain the higher temperatures, but it has only been of late years that the attention of the physicist has been called to the increasing necessity of reaching the lower grades of temperature. Consequently our applications to this result have been more or less crude.

In our own climate the uniformly low temperature of winter obviates recourse to artificial refrigeration during several months of the year, and even at present the most usual source of refrigeration during the summer months is from the storage of large amounts of ice, which, by different means, serves to maintain a sufficiently low degree of cold during the necessary time.

But there are other climates than our own, where, from their very nature, the demand for low temperatures is more strenuous.

Although nature manufactures ice for nothing, still it is a costly article when placed in the market. The expense of harvesting and transportation, and storage, and the waste of handling and melting are enormous, even in this country where nature is so prodigal. What must they be then in a zone where there is no ice, and which has to depend for its supply upon countries thousands of miles distant?

The equivalence of heat and mechanical energy is well known, and at first thought the production of cold ought to be capable of immediate and complete success, but, as will be seen, the means at our disposal present difficulties which as yet are many and great.

All systems of refrigerating machinery of to-day are based upon the same principle, viz., the volatilization and subsequent condensation of a liquid, or the converse of a gas.

The volatilization produced by vacuum-pumps, causing a reduction of temperature by the absorption of latent heat; the condensation produced by mechanical power causing compression, the resultant heat being carried off by cold water.

The properties of a liquid or gas are greatly restricted by the requirements of a freezing agent.

It must obviously:

1. Be volatile at low temperatures, and at a pressure not much below that of the atmosphere, for otherwise we could not attain low degrees of cold, and we should have difficulty in preventing the admission of air into the pumps while working under a vacuum.

2. Not reach high pressures even at high temperatures, as in hot climates, for high pressure means leaking joints and extra power.

3. Be stable in composition after frequent evaporations and condensations. Some of the freezing agents now in use have been found to be so impure of manufacture, that after working but a short time, with but little vacuum and low pressure, the impurities which for a time caused these results, gradually separated, leaving a gas requiring very high pressures for condensation.

4. Have no action on lubricating substances.

5. Have no action on metals used in machinery.

6. Be uninflammable, and non-explosive.

The machine must be simple of construction, small in plant, and efficient in power per ton of fuel consumed.

And lastly, the cost of production of cold or ice must be small.

Let me briefly review a few of the systems hitherto used in the manufacture of artificial ice.

COMPRESSED AIR MACHINES.

In this system advantage has been taken of the compression and subsequent expansion of air. If air at ordinary temperature and atmospheric pressure is brought rapidly to a certain tension its temperature rises correspondingly, but if, during this operation, it is cooled to its initial temperature, and then allowed to expand until the atmospheric pressure is again attained, its temperature is sensibly lowered from the point at which it has been cooled to a figure as much below as the compression has raised it above the initial temperature. This, in short, is the working principle of the

system. Simple as it seems, and inexhaustible and inexpensive as is the freezing agent, it has given less satisfactory results than any other system in use. The poor conductibility of air requires large vessels to abstract its heat; for in transmitting heat we must deal in masses, not in volumes. Moreover we experience great difficulty in dealing with the moisture held in the air. This becomes frozen and clogs the valves and pipes of the machine. It has been proposed to dry the air or to use the same over and over again, but the mechanical difficulties to be surmounted are so enormous, that, practically, such treatment is impossible. Glycerine only can be used as a lubricant.

AMMONIA MACHINES.

These employ either a saturated solution of ammonia in water or liquefied ammonia-gas. The former being the agent of the Carré machine, the latter of the Tellier system. These machines, though giving remarkably low temperatures, are subject to the disadvantages of high pressures. Gaseous ammonia requires to be compressed from 180 to 250 pounds per square inch, and sometimes even more, to bring it to a liquid state at a temperature of 75°-80° F., which is about that of running water in warm climates in the best condition. Under such pressures the joints are kept tight with difficulty, causing a costly loss of gas. Great power is necessary to drive the machinery. No grease can be used in lubrication, being immediately saponified by the ammonia. The action of ammonia on copper and cast iron (owing to pressure) is another objection.

ETHER MACHINES.

As is well known, ether has an unusually low tension—two or three pounds at 27° F. This requires the use of a pump working almost under a vacuum. The entrance of air, under these circumstances, into the stuffing-box and other joints, is difficult of prevention. The ether, therefore, becomes oxidized, and the working of the machine impaired. This product is not stable of composition, and soon is decomposed into isomeric compounds less volatile. No grease can be used as a lubricant, as it is soluble in ether. Leaks about the machine are very dangerous, as the vapors of ether are highly inflammable. Pressures are not so great as in the machines just mentioned, seldom exceeding 100 pounds per square inch.

PETROLEUM-DERIVATIVE MACHINES, SUCH AS CHYMOGENE, GASOLENE, ETC.

These have all the disadvantages of the above machines, viz., low tension, inconstancy of product, high inflammability, etc.

ANHYDROUS, SULPHUROUS DIOXIDE MACHINES.

M. Raoul Pictet, of Geneva, made a long stride forward in the perfecting of ice-machinery, in producing his system of using sulphurous dioxide. It is liquid at 14° F., and at 60°-65° F. has a pressure of from three to four atmospheres. It has no effect upon grease, and is in itself a lubricator. It has no effect upon metals, and the product is perfectly stable, and is not inflammable; in fact, as is well known, it even stops combustion. This product must, however, be anhydrous. Water immediately transforms it into sulphuric acid. Any minute blow- or pinhole in a casting will give advent to moisture producing corrosion, and the hole gradually increases until, under the internal pressure, the gas escapes. This accident has happened frequently to machines of this system.

After the perfection of this latter system, the question arose as to whether or not it would be possible to combine at the same time, the low pressures of certain volatile liquids with the greater cooling properties of others. This seemed to insure an increased production for the smallest amount of mechanical energy expended, and to avoid the principal difficulties mentioned. Such a machine would not be too cumbersome, consequently not too costly; the low pressure at which it would work would insure the continuity of its running, and a minimum, if not the exclusion of loss by leakage. If, moreover, the liquid employed could have no action upon metals or greases, or be self-lubricant, and be uninflammable, such a liquid would seem to cover most of the necessary requirements for an economical and effective cold-producing agent.

Working upon these ideas Messrs. Tessie Du Motay and Auguste J. Rossi, both chemists of New York city, found that many ethers, as well as their alcoholic radicals, possessed an absorbing power for gaseous sulphurous anhydride which, for some of them, amounts to nearly three hundred times their volume of gas. The experiments extended to other bodies besides the ethers and alcohols, and chloroform, bisulphide of carbon, petroleum, naphtha, and many of the hydrocarbons absorbed sulphurous oxide, though to a less amount than the ethers.

They obtained a liquid as a result of their experiments, which had no pressure at 60° Fahr., and many of the least volatile had no pressure at 90° Fahr.

The property of inflammability of the ethers was so counteracted by the presence of the sulphurous dioxide that for many it had entirely disappeared, while with some of the most volatile and inflammable hydrocarbons, when saturated, it was only after raising the temperature and disengaging the sulphurous dioxide that a continuously impinging flame produced but intermittent flashes.

One of these liquids Messrs. Du Motay and Rossi then chose as the agent for their system of ice-machinery. It was ordinary ether charged to half-saturation with sulphurous dioxide. With this "binary liquid" intense cold is produced, greater in fact than the sum of the cold which could be produced from the evaporization of each constituent taken alone, if we admit the theory of St. Clair Deville on the dissociation of elements and certain laws of thermo-chemistry.

The manner of working of this machine I will give succinctly as follows:

The refrigerator (Fig. 64) in which the charge of binary liquid is contained is in form similar to a copper tubular boiler. This refrigerator lies in an iron tank. It is put in communication by means of proper pipes, with an aspiration and compression gas-pump, receiving its action from a steam-engine placed on the same bed-plate. This gas-pump is connected with a tubular condenser (Fig. 65), contained in an iron tank and cooled by a circulation of water. It is connected with the refrigerator by a small copper pipe for the return of the binary liquid when reformed. The pump being started, the binary liquid in the refrigerator vaporizes. The vapors enter the gas-pump, then are compressed and delivered into the condenser. The ether liquefies and absorbs anew the sulphurous dioxide, reconstituting the binary liquid, which is returned by the small pipe between the refrigerator and condenser to the refrigerator, to be again evaporated, and so on. The refrigerator-tank contains a brine solution which covers the refrigerator. The brine flows from this tank into a large iron tank, in which are immersed the galvanized iron cases or moulds which are filled with the water to be frozen.

A circulating pump returns the brine to the refrigerator-tank, where it is cooled, and whence it flows again into the large tank. Now, each time the binary liquid evaporates under the vacuum

made by the gas-pump in the refrigerator, it absorbs for its latent heat of volatilization the sensible heat contained in the brine which surrounds the refrigerator, which brine abstracts it, in its turn, from the water in the moulds, which ultimately freezes. A small valve regulates the returning flow of binary liquid from condenser to refrigerator, and two globe valves, one on the refrigerator and one on the condenser, serve to cut the communication between these vessels and the gas-pump.

From this brief description of the machine it can be seen that it is in its construction very much the reproduction of the long and well-known type of ether-machine, with few modifications of details. In fact, any machine using a refrigerator, a condenser, and a double-acting gas-pump, could be adapted to this new system, but with different results according to the liquid used. Liquid ammonia-machines, for instance, would have entirely inadequate gas-pumps, though steam-power would be too large; they would require very serious modifications. Sulphurous dioxide, methylic ether, and chloride-of-methyl machines would necessitate a gas-pump double in size for the same production. The steam-cylinder would be ample, and the refrigerator and condenser could be used, though rather small. If nothing was changed in a Pictet machine working with the binary liquid, it would produce one-half or two-thirds of its original production, but with less than one-half or two-thirds of the power required when sulphurous dioxide alone was used, so that, in short, the production of ice per pound of coal consumed would be larger. But the machine the best adapted to run by the new system without serious change of any kind would be the ether machine. The specific gravity of the ether being .725 against .950 to .980 for the binary liquid, both refrigerator and condenser would be ample for nearly a double production. The dimensions of the gas-pump would be also suitable. At each stroke it would evaporate a quantity of binary liquid, giving in gas the same volume as that of ether; but this evaporation would produce an intensity of cold theoretically more than double that produced by ether alone. The pressures in the condenser not being sensibly different from those of ether, it is safe to say that ether-machines, with little or no alterations, could by this system be made to nearly double their production, with many other incidental advantages, such as avoiding the difficulty of greasing, and the possible dangers of the vapors of ether.

It can then be easily seen that for the mechanical work of com-

pression necessary to liquefy the dioxide is substituted a chemical power of affinity and absorption of the liquid ether or gaseous sulphuric dioxide. Such a machine, therefore, ought to work as easily and continuously as an ether-machine.

The dioxide is a perfect lubricant, no grease having been used in any of the gas-pumps now in use. When running, the pressures vary from 10 to 15 pounds per square inch, when at rest from 0 to 2 pounds.

Should, in presence of water, and by its action, the sulphurous dioxide be transformed into sulphuric acid, the further action of this acid on ether would produce sulphovenic acid, a very weak acid to say the least, and of which the action upon metals is insignificant if not absolutely *nil.*

Having explained the principal features of the working of the system, it may prove interesting to examine the product, and to give a few facts based on actual observation while one of the machines was running at the Delamater Iron Works during the summer and fall of last year.

After filling the cans they are immersed in the bath, and the temperature of the contained water immediately begins to fall. The length of time necessary to produce any changes in temperature, etc., depends on temperature of bath, temperature of immersed water, and many other circumstances. After the water has reached a temperature of 32° Fahr., a thin coating of ice is observable on the sides of the cans. This gradually increases on the four sides and bottom, the five surfaces approaching each other (Fig. 66), until the cake is finally closed. Sometimes the freezing takes place in the form of long needles projecting into the centre of the can as in Fig. 67. These gradually thicken at their base until the whole mass is frozen solid. These two forms are sometimes noticeable in cans which are side by side. On removing the cans when the cakes are completed, no difference in appearance is visible.

When the bath is at a low temperature, say 24° Fahr., the cakes have an opaque, white appearance, and all are uniformly frozen, but when the bath is at a higher temperature, say 28° Fahr., cans are often found after long immersion which show not the slightest signs of congelation. This phenomenon is known as the "surfusion of the ice." A slight agitation of the water will immediately cause the formation of ice crystals, and the freezing continues as with the other cakes.

It is observable that the higher the temperature the more translucent the cake of ice appears. It is accounted for by the slower formation of the ice and consequent expulsion of the air. Water boiled for a few moments gives a beautifully translucent cake. Whatever organic matters the water may hold in solution are invariably found in the centre of the cake.

Special processes for filtering and deaërating water have been more or less successful, and to obtain perfectly translucent ice is simply a matter of dollars and cents.

Direct experiments made to ascertain the quantity of heat by the calorimeter necessary for the fusion of one kil. of ice, either artificial, natural, opaque, milky or transparent, have failed to show the slightest difference.

The bad conductibility of the ice itself is very apparent. Holes 10″ deep and 1″ apart were drilled in a block of solid ice immediately on being taken from the bath. In these holes thermometers were placed. At 1″ from the sides of the box the temperature observed was 22° Fahr., at 2″ 27° Fahr., at 3″ 30° Fahr., in centre 32° Fahr. The bath at the time being 18° Fahr. Thus showing that in the centre the water was frozen from contact with the surrounding ice and not from the direct effect of the bath. This shows the advantage of making thin cakes of ice.

An experiment was made to ascertain the curve of congelation and the thickness of the ice formed every hour for twenty-four hours. The result I append in the table, and the curve (Fig. 68) will show how great the non-conductibility of the ice lengthens the time of freezing. There are many variations in the curve due to variations in the temperature of the bath from 21°–25° Fahr.

It will be noticed that this curve is similar to the curve formed by the interior of the ice while freezing in the can. (See Fig. 66.)

While freezing, the expansion of the ice forces the water up through the centre of the can above the surface of the bath, where it freezes in parallel laminæ.

If, however, the can has been so filled with water that the level, after expansion, does not rise above this point, the *v* noticeable in the diagram closes, and no horizontal layers are formed.

TABLE.

A. represents thickness formed, wide side.
B. represents thickness formed, narrow side.
Size of cans 6″ x 12″.

	A.	B.
10 A.M.		
11 "		.2375
12 M.	.4312	.5500
1 P.M.		.7687
2 "	.7812	.9375
3 "		1.0937
4 "	1.1500	1.1875
5 "		1.2812
6 "	1.3250	1.3312
7 "		1.3937
8 "	1.5006	1.4315
9 "		1.5125
10 "	1.6375	1.5687
11 "		1.6250
12 "	1.7625	1.6750
1 A.M.		1.7250
2 "	1.8685	1.7625
3 "		1.8250
4 "	1.9875	1.8437
5 "		1.8637
6 "	2.1250	1.9062
7 "		1.9312
8 "	2.1875	1.9562
9 "		
10 "		

Let us pass for a moment to the theoretical working of this system.

The gas enters the pump from the refrigerator at a pressure P, and temperature t, the temperature of ebullition. It is then forced into the condenser at a pressure P^1 and temperature t^1 corresponding to the temperature of the circulating water. Here it is reduced to a liquid state again, and returns to the refrigerator at the original pressure P to be volatilized anew. The pressure in the condenser is due to the work of the pump, and the heat generated during the compression of the vapors from P to P^1 is carried away by the circulating water. Hence in the outflowing water must be found the heat corresponding to the horse-power of the engine plus the heat resulting from the latent heat of liquefaction of the gas. But as there returns to the refrigerator a weight of liquid equal to that which has been volatilized, there will be required an absorption of heat equal to the latent heat ceded to the water of

the condenser minus whatever excess of heat may be brought back by the liquid in returning to the temperature t.

Hence an approximate value of this amount of heat available to make ice will be found by subtracting from the total amount of heat carried away by the condensing water, the thermal equivalent of the horse-power necessary to produce the compression.

This disposes of all objection which has been raised by some engineers that the amount of ice produced by the machine could correspond only to the thermal equivalent of the horse-power consumed to produce the compression.

The following figures taken but a few days ago from a three-ton machine now running for special purposes of manufacture will illustrate.

Example.—There was in the refrigerator-tank when started 14,200 pounds of chloride-of-magnesium brine, and of the substance to be cooled in the cans, 4750 pounds. In all 18,950 pounds of liquid to be cooled. The whole mass at 8 A.M., the time of starting, was at a temperature of 49° Fahr. At 2 P.M., or in six hours, the temperature fell to 28°, or at the rate of 3.4° per hour. 14,200 pounds of brine with a specific heat of .85 compared to water, would for 1° represent 12,070 thermal units, the contents of the cans with a specific of .9 would give 4275 thermal units. Their sum multiplied by 3.4°, the fall of temperature per hour, gives as the work of the refrigerator per hour 57578.4 thermal units.

On the other hand, there was passing through the condenser-tank from actual measurement 540 gallons of water per hour, or 4500 pounds. The temperature of this water was raised from 50° to 67° or 17°. Thus the work of the condenser represents 76,500 thermal units.

The horse-power taken from the gas-pump at the time, showed 7 horse-power.

Now, 1 horse-power = 33,000 foot-pounds per minute, or 1,980,000 foot-pounds per hour.

772 foot-pounds = 1 thermal unit.

Hence 1 horse-power = 2565 thermal units per hour. 2565 × 7 = 17,955 thermal units as the work of the pump.

The total work, shown above, of the condenser of 76,500 thermal units minus the work of the pump, or 17,955 thermal units, gives us the work of the condenser in reducing the gas to a liquid. This is 58,545 thermal units. We found in the refrigerator 57,578 thermal units. The coincidence is close.

Reckoning on the *total* horse-power of the engine as 12, we would have only 30,780 thermal units, as against 57,578 thermal units.

The work of simple compression exerted by the engine, viz., 17,955 thermal units, would correspond to about one ton of ice, whereas we obtained actually over three tons.

Knowing the pressures P and P^1, and their corresponding temperatures t and t^1, we can easily calculate the horse-power necessary to produce the compression.

We suppose the gas to be compressed isothermally. It is sufficiently correct for our examination. Then it follows the hyperbolic ratio, and the amount of work required to reduce a given volume V of gas at pressure P, to a smaller volume V^1 corresponding to the given superior pressure P^1, and to force it into the condenser at that pressure is in form of the initial work.

$$T \textit{ foot-pounds} = PV\left(1 + \textit{hyp. log.}\ \frac{P^1}{P}\right).$$

When V is the initial volume at $t°$ F. at pressure of refrigerator ($2\frac{1}{2}$ pounds normal), P^1 the final pressure in condenser (35 pounds normal),

$$V = V^1\,(1 + K)\,(t - 32) \qquad K = \frac{1}{493.2} = \text{coefficient of expansion.}$$

$$V = V^1\left[\frac{493.2 + (t - 32)}{493.2}\right]$$

We make the calculation of the work of the pump for 1 pound of liquid. 1 cubic foot of air at 32° F. weighs 0.0807265. Let δ = the specific gravity of the gas at 32° F., 1 cubic foot of gas weighs, therefore, at 32° F., $0.807265 \times \delta$, and 1 pound occupies at 32° F. a volume of $\frac{1}{.0807265 \times \delta}$, and for $t°$ a volume of $\frac{493.2 + (t - 32)}{.0807265 \times \delta \times 493.2}$.

Hence, for one pound,

$$T \textit{ foot-pounds} = \frac{360\,[493.2 + (t° - 32°)]\left[1 + \textit{hyp. log.}\,\frac{P^1}{P}\right]}{.0807265 \times \delta \times 493.2}$$

Letting λ = the latent heat of the gas in thermal units, and letting c = its specific heat under constant pressure. $t° = 20°$ actual or 481.2° absolute $\therefore\ t° - 32° = 489.2°$.

Evidently we have the heat absorbed in the refrigerator per pound of liquid volatilized.

$$Q = \lambda - c\,(t^1 - t).$$

Now letting $P = 4.8$ be the weight of liquid volatilized per minute, we have

$$Q = \lambda - c\,(t^1 - t) \times P \text{ and}$$

$$T \textit{ foot-pounds} = P\left[\frac{360\,(493.2 + (t° - 32°)\,)}{0.807265 \times \delta \times 493.2}\right] \times \left[1 + \textit{hyp. log}\ \frac{P^1}{P}\right].$$

We have here all the elements of our calculation.

The expense of making ice by this system is estimated to be as follows:

Three-Ton Machine.

One man, day, \$1.50; one man, night, \$1.50, ...	\$3 00
One engineer acting as fireman, day, \$2.75; night, \$2.75,	5 50
Coal, $\frac{3}{8}$ ton. at \$4.00 per ton, ...	1 50
Total cost of three tons of ice, ...	\$10 00
or, \$3.33 per ton.	

Six-Ton Machine.

Men at same rate as above, ...	\$8 50
Coal, $\frac{3}{4}$ ton at \$4.00 per ton, ...	3 00
Total cost of six tons of ice, ...	\$11 50
or, \$1.92 per ton.	

Ten-Ton Machine.

Men and engineers as above, ...	\$10 50
Coal, one ton at \$4.00 per ton, ...	4 00
Total cost of ten tons of ice, ...	\$14 50
or, \$1.45 per ton.	

Twenty-five-Ton Machine.

Men and engineers as above, ...	\$13 50
Coal, two and a half tons as above, ...	10 00
Total cost of twenty-five tons of ice, ...	\$23 00
or, .92 per ton.	

Fifty-Ton Machine.

Men as above, ...	\$20 00
Coal as above, five tons, ...	20 00
Total cost of fifty tons of ice, ...	\$40 00
or, .80 per ton.	

Ice-machinery has more than a commercial interest, and many experiments of a scientific nature have been made possible by its means. I append the results of a few experiments performed by M. Raoul Pictet, of Geneva, the inventor of the system of sulphurous dioxide machines which bears his name. It would be evidently anomalous to the subject to enter further into their detail. They are added simply for reference.

1. For all liquids the cohesion is constant.

2. The internal latent heat of all liquids referred to one and the same pressure, multiplied by the atomic weight taken at a uniform temperature, gives a constant product.

3. For all liquids the difference of internal latent heats at any two temperatures, multiplied by the atomic weight, gives a constant product.

4. The derivative of the Napierian logarithm of the ratio between tensions and temperatures is constant for all liquids when they are compared under the same pressure and temperature.

5. The latent heats of all liquids are multiples of their specific heats.

For those who desire to see the discussion of the above principles, or pursue the subject more in detail, I would refer them to a pamphlet by M. Pictet, on *The Applications of the Mechanical Theory of Heat to Volatile Liquids.*

With regard to ice-machinery, experience alone can reach its importance and value. The Binary Absorption System, I present as a step forward in its development. It is now one year since the first machine was started at the works of Delamater & Co., and we are constantly receiving favorable reports from representatives of the machine, working under the climates of North and South United States, Cuba, Mexico, and Japan. These reports serve only to justify the soundness of the working principle of the machine.

I will only mention its numerous applications to the cooling of air or liquids for technical purposes, such as for breweries, cold storage-houses, chemical works, public buildings, and the preservation of meats on board of steamers and cars.

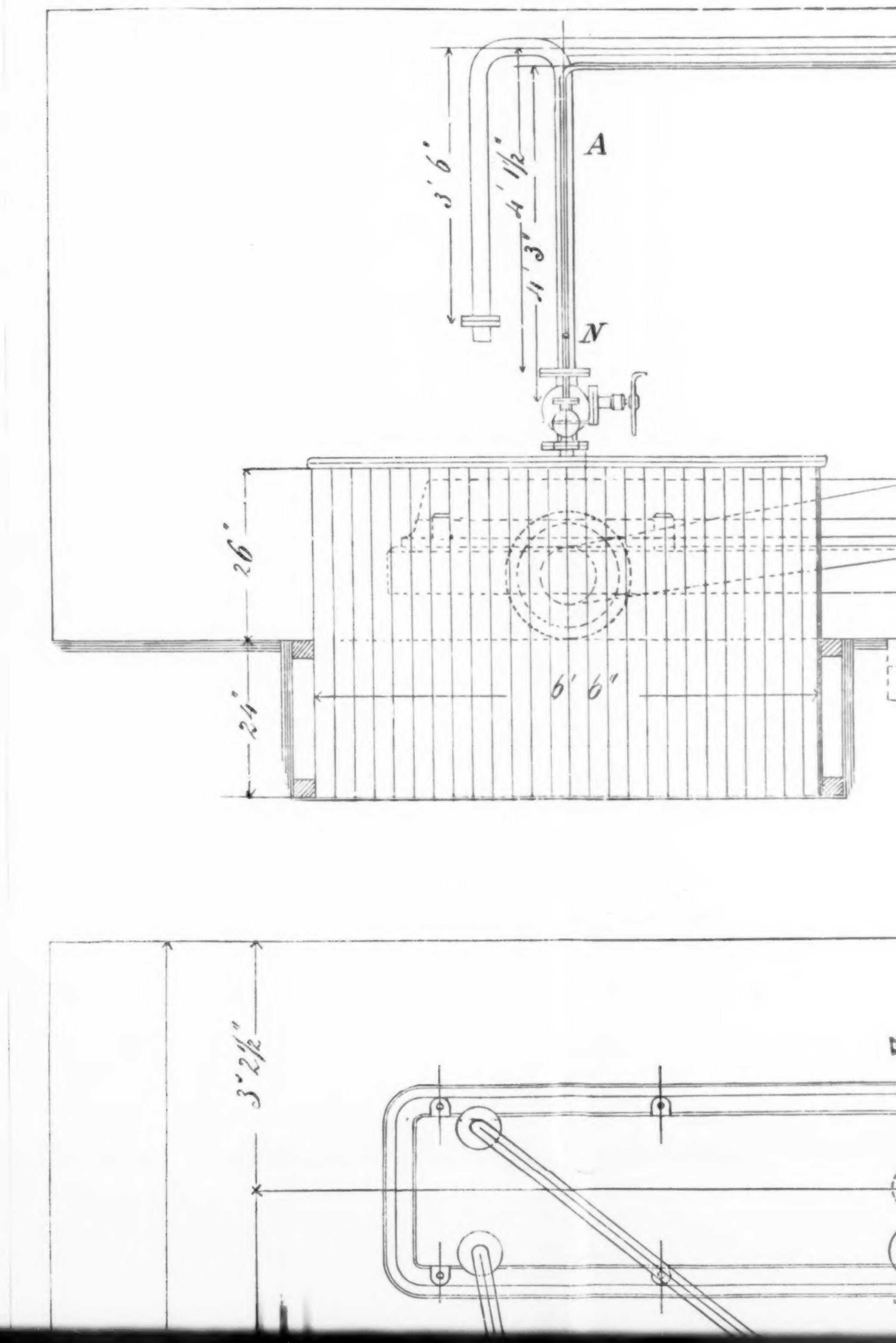
I have endeavored to present to you a collection of practical facts from my own observation, and from experiments conducted under the eye of the inventors, and to give a *résumé* of the principal scientific questions involved in the running of such machines, quoting freely and textually sometimes, from such a competent authority as Mr. Rossi himself.

XXXVII.

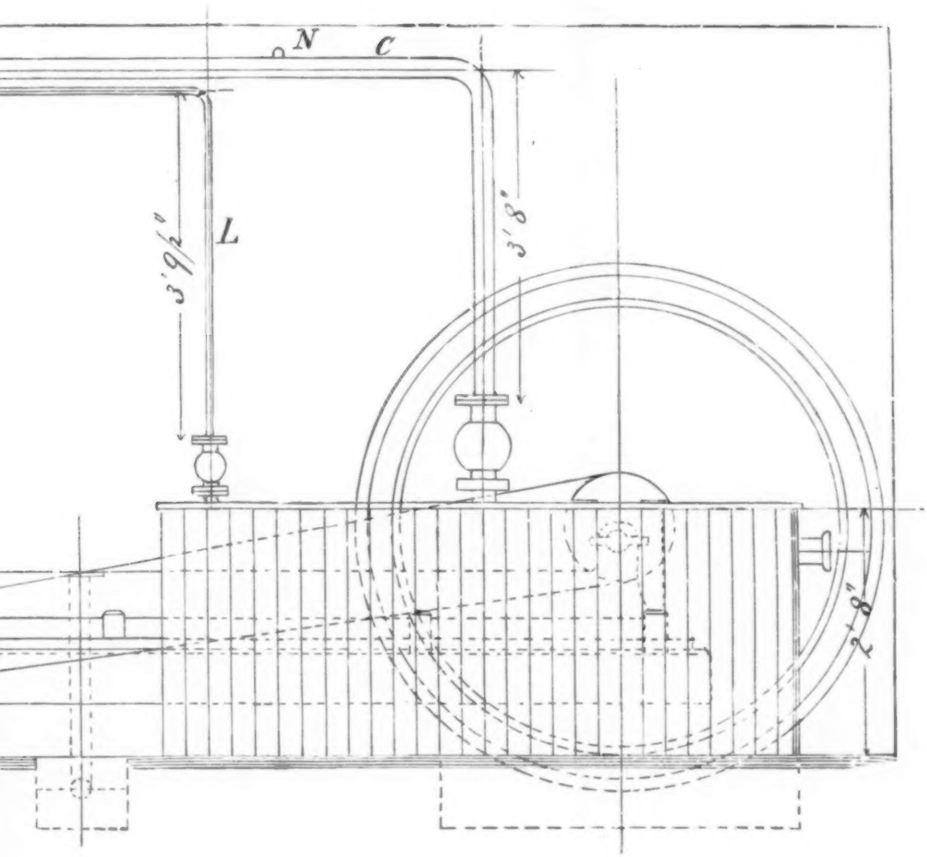
EXPERIMENTS ON THE ADHESION OF LEATHER BELTS.

BY SAMUEL WEBBER, MANCHESTER, N. H.

THE following experiments have been made with the view of adding somewhat to our actual stock of knowledge of the adhesion of leather belts to smooth cast-iron pulleys, at different arcs of surface contact, and although not as complete as might be wished,

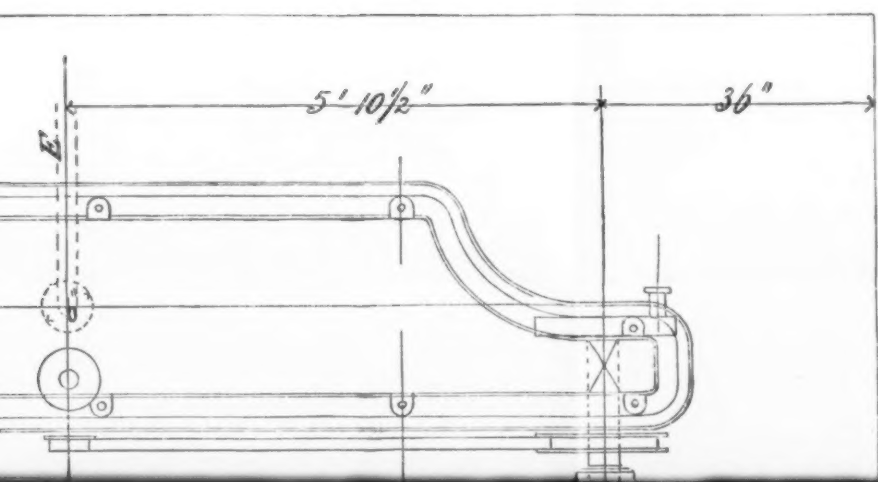


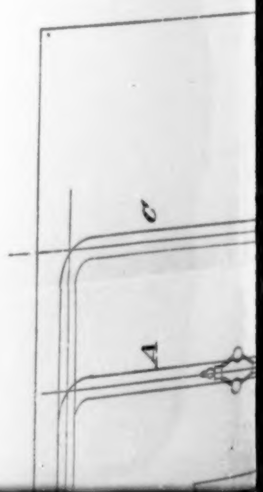
A
N
3' 6"
4' 1½"
4' 3"
26"
24"
6' 6"
3' 2½"

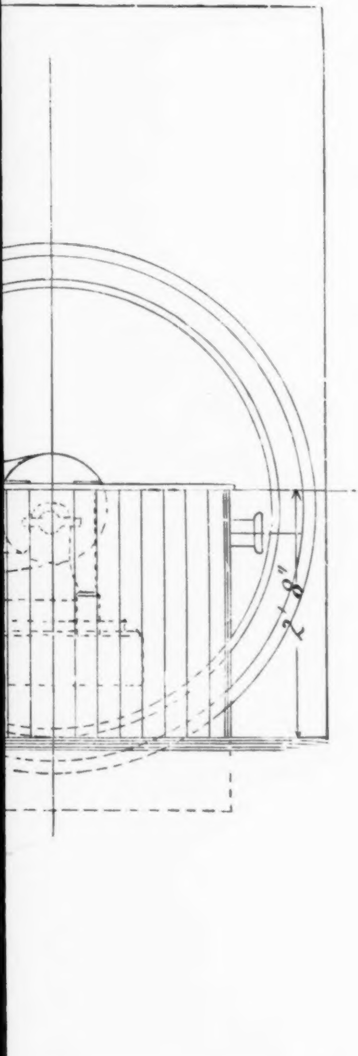


PLAN

2-TONS

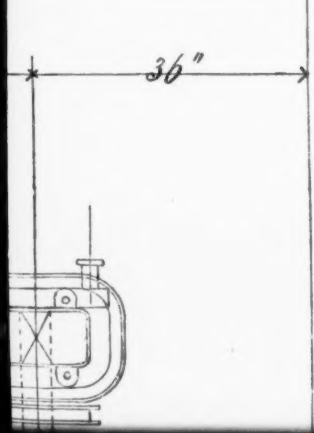






PLAN AN

2-TONS IC



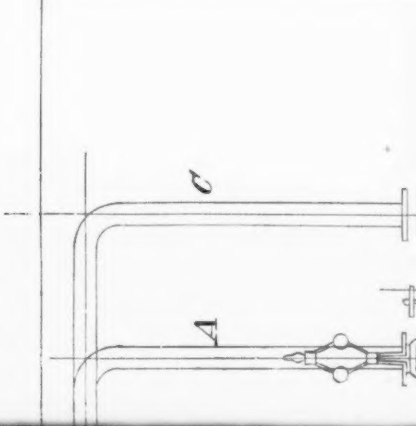


Fig. 61.

AND ELEVATIONS

OF

ICE MACHINES.

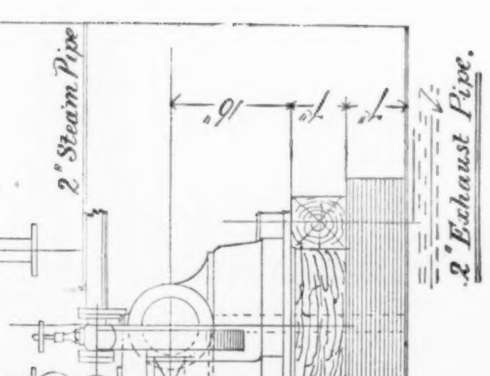
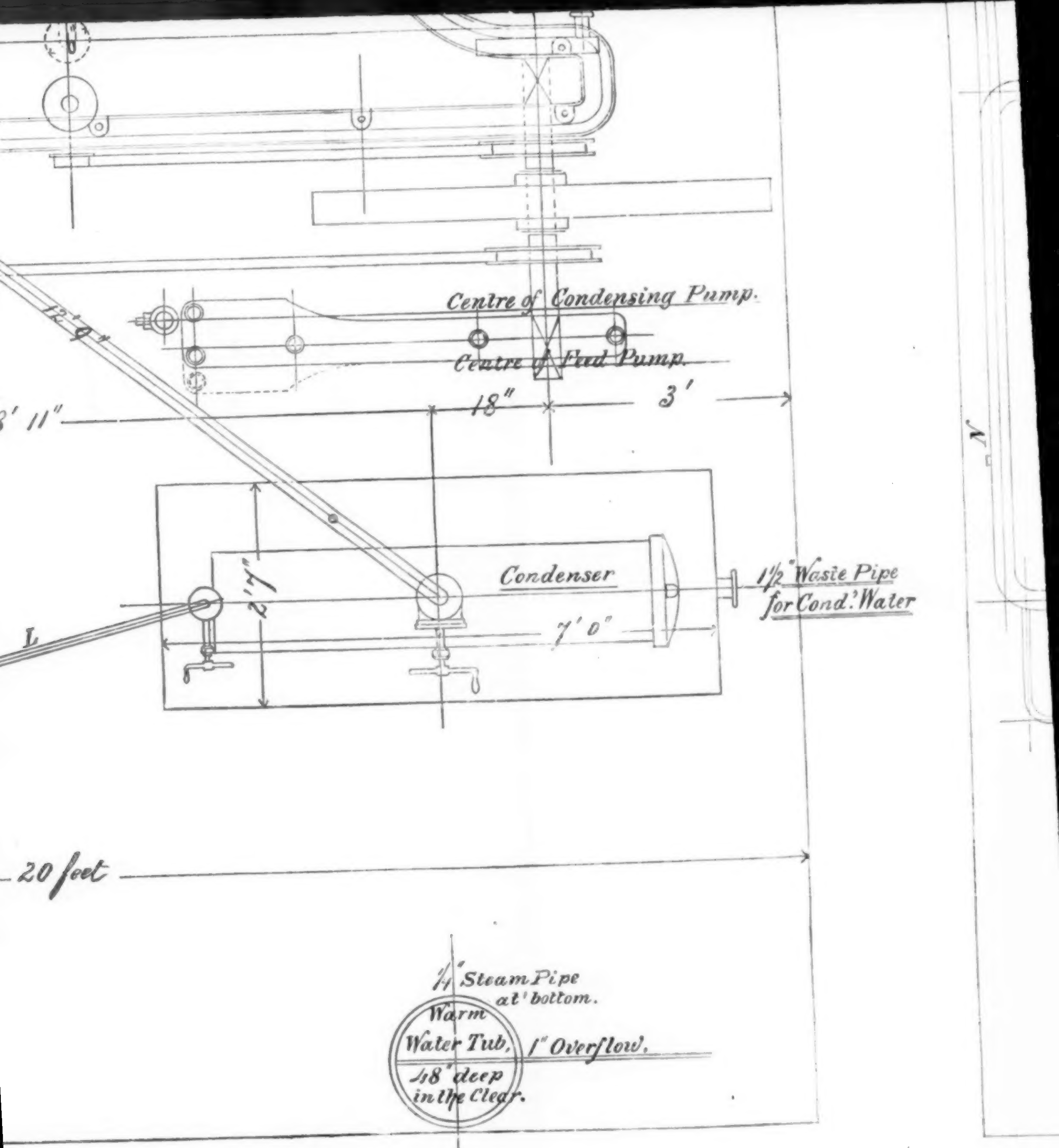


Fig. 62



12' 9"
Centre of Condensing Pump.
Centre of Feed Pump.
3' 11"
18"
3'
Condenser
2' 7"
L
7' 0"
1½" Waste Pipe for Cond.' Water
20 feet
¼" Steam Pipe at bottom.
Warm Water Tub, 48" deep in the Clear.
1" Overflow.

ig. 62.

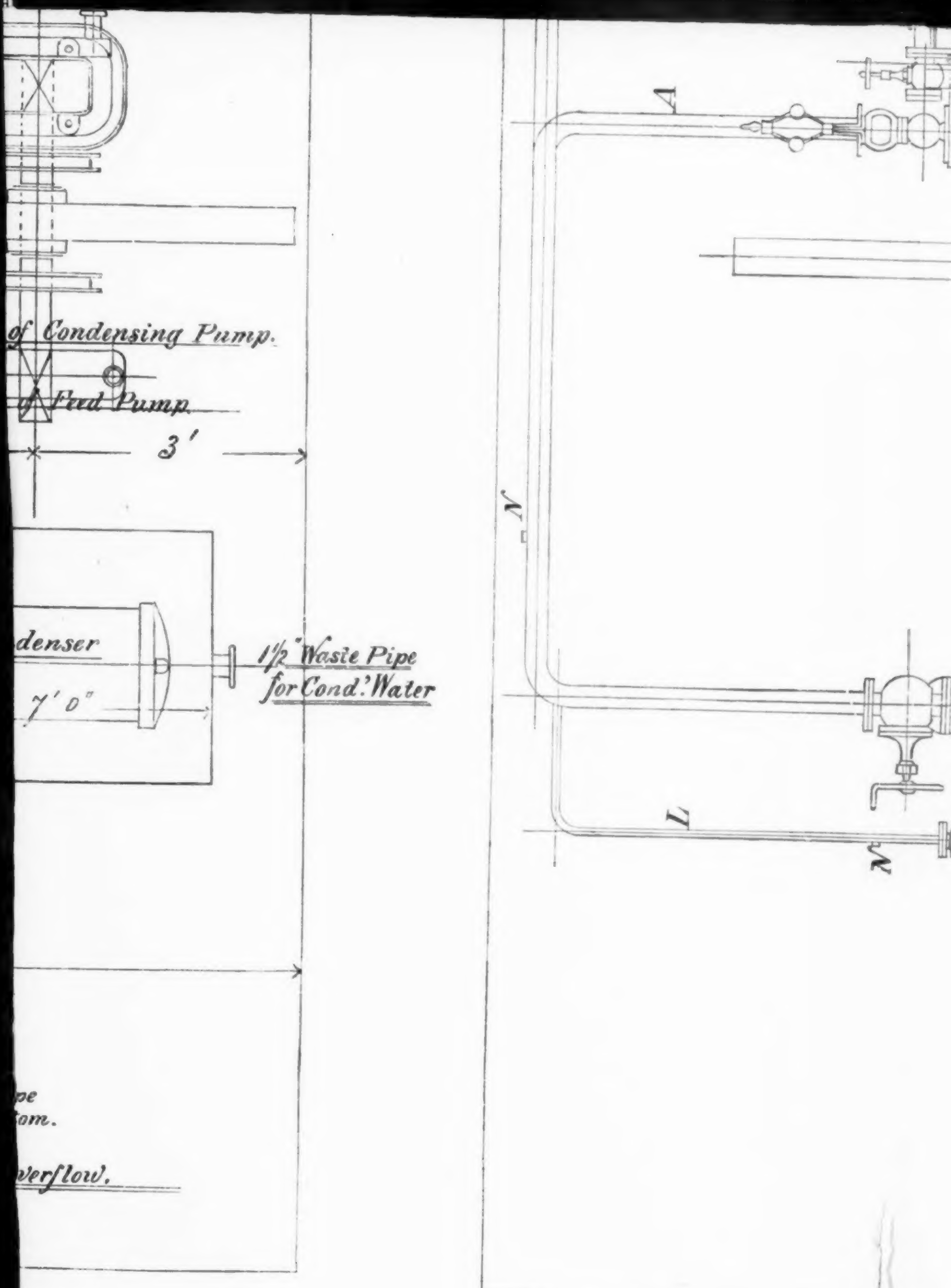

of Condensing Pump.
of Feed Pump.
3′
denser
7′ 0″
1½″ Waste Pipe
for Cond.' Water
pe
tom.
verflow.
A
N
L
M

Fig.

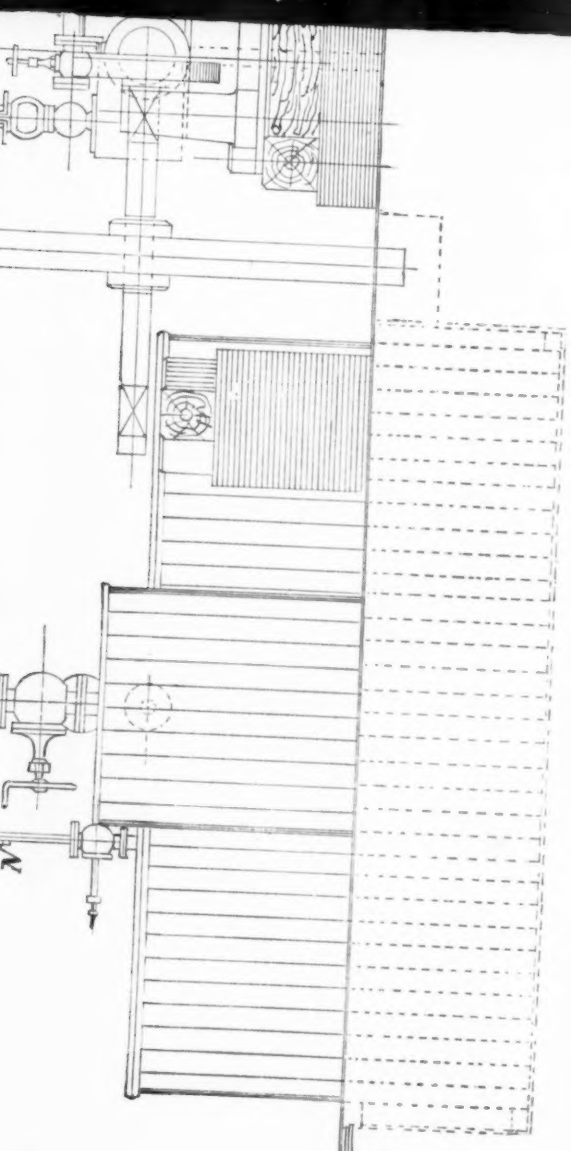


Fig. 63.

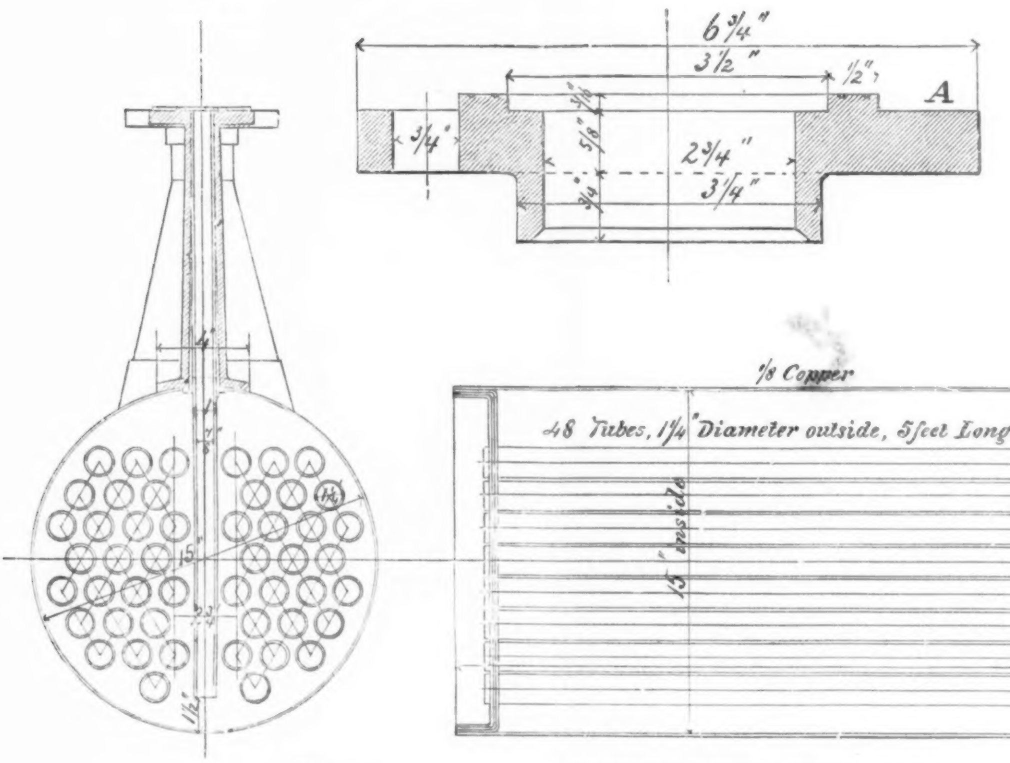


Fig. 64.

REFRIGERATOR FOR 2-T
BINARY SY

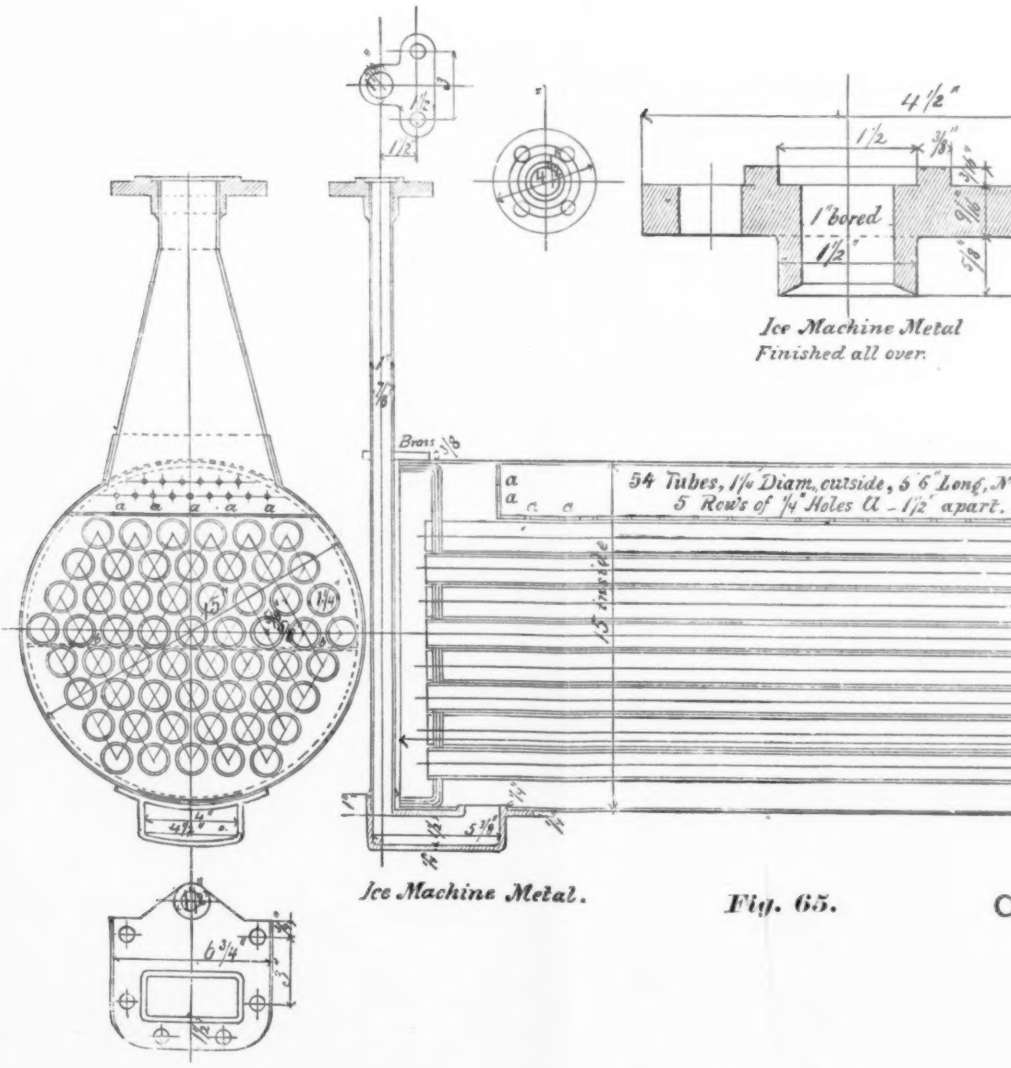
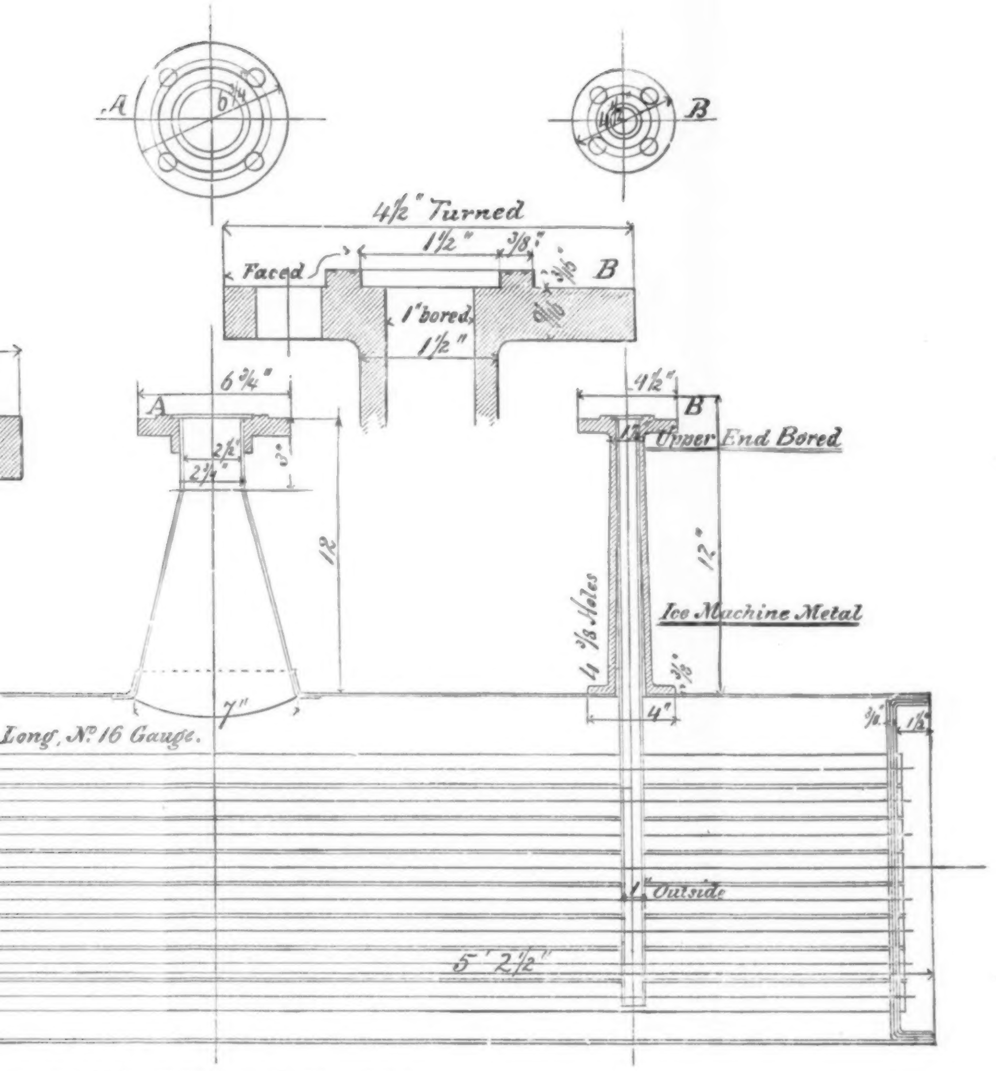


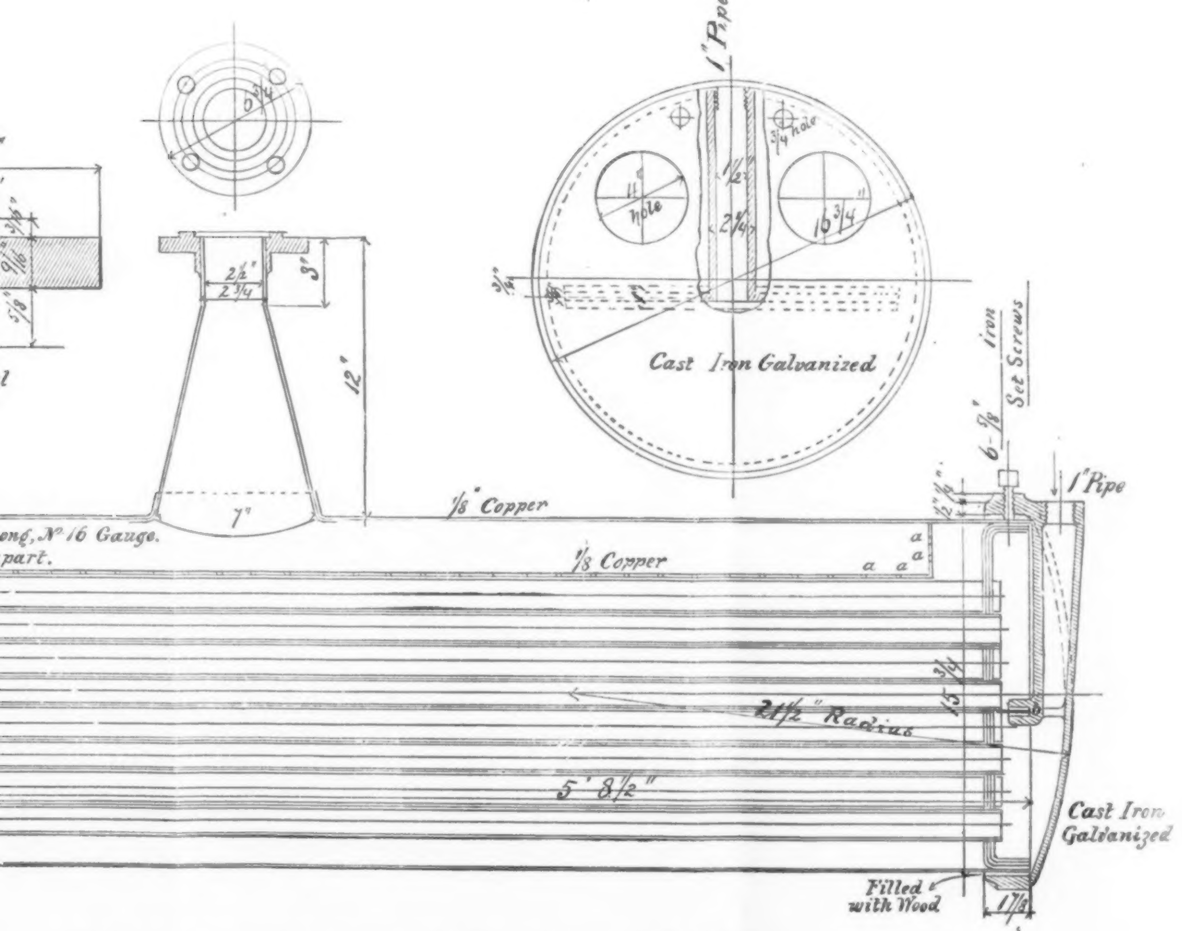
Fig. 65.

C



2-TONS ICE MACHINE.
SYSTEM.

Scale, 1½ ins. = 1 Foot.



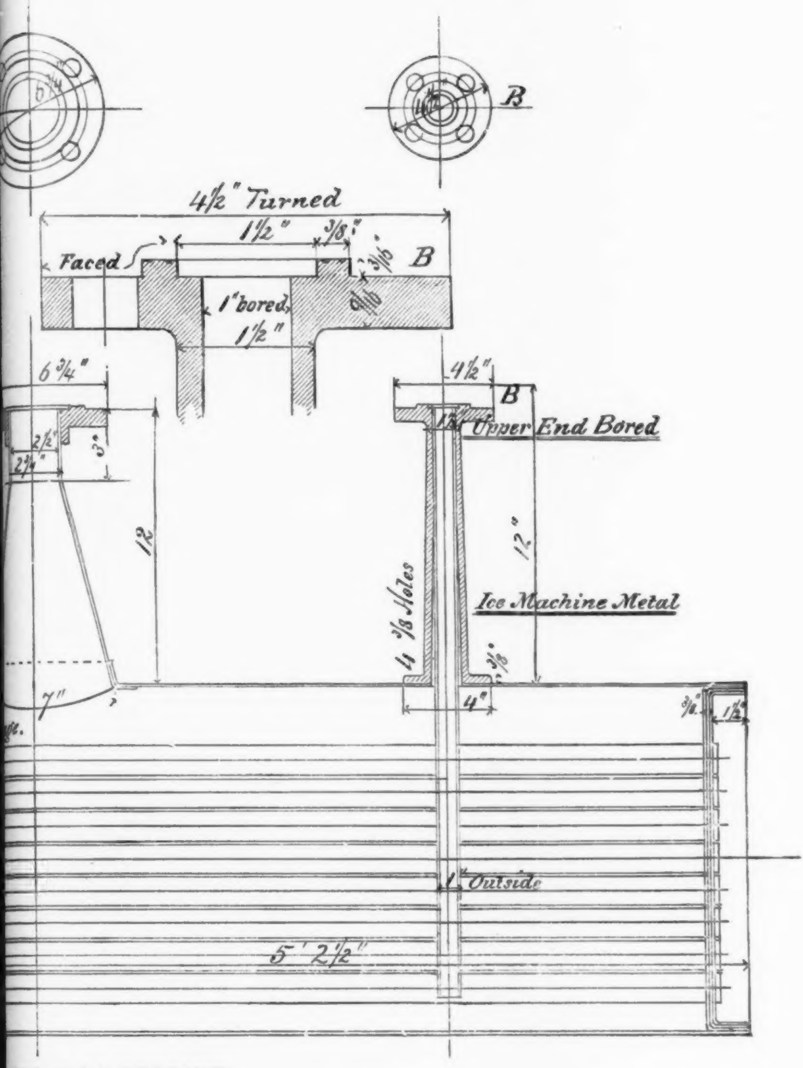
CONDENSER FOR 2-TONS ICE MACHINE,
BINARY SYSTEM.

Scale, 1½ ins. = 1 Foot.

Fig.

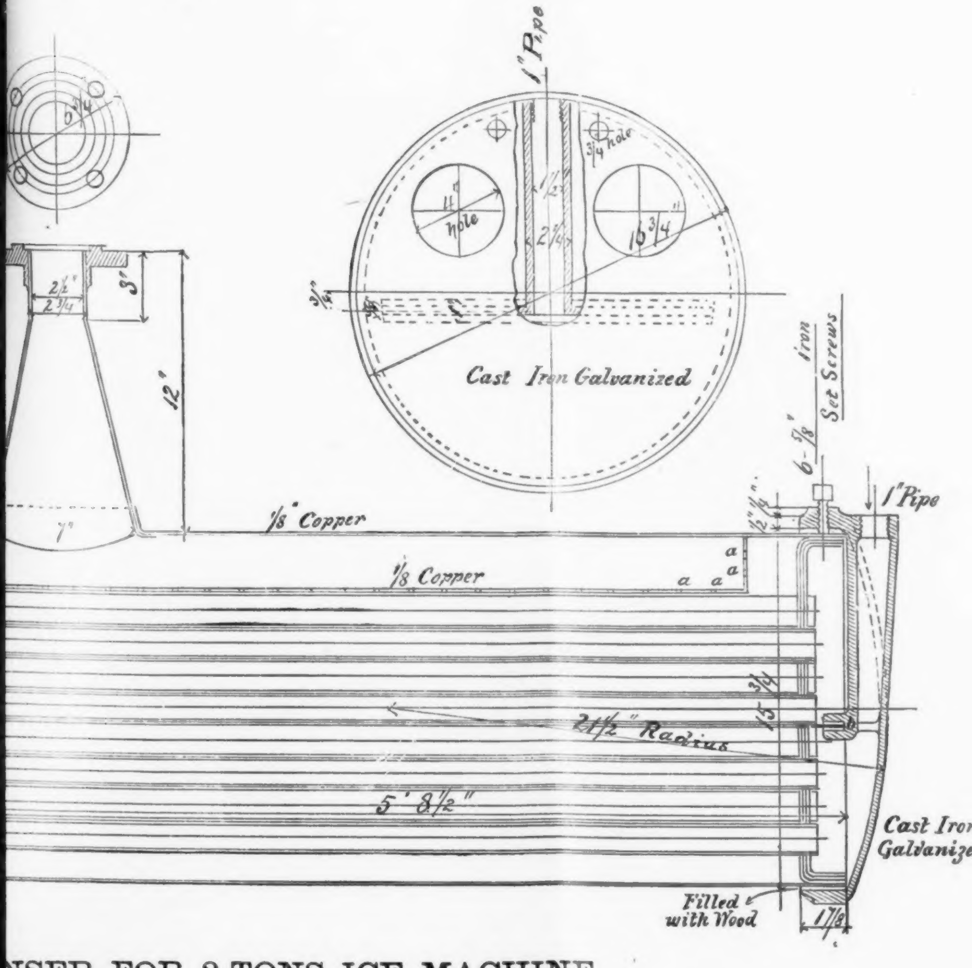
Fig. 67.

C



CE MACHINE,

ins. — 1 *Foot.*



NSER FOR 2-TONS ICE MACHINE,
BINARY SYSTEM.

Scale, 1½ *ins.* — 1 *Foot.*

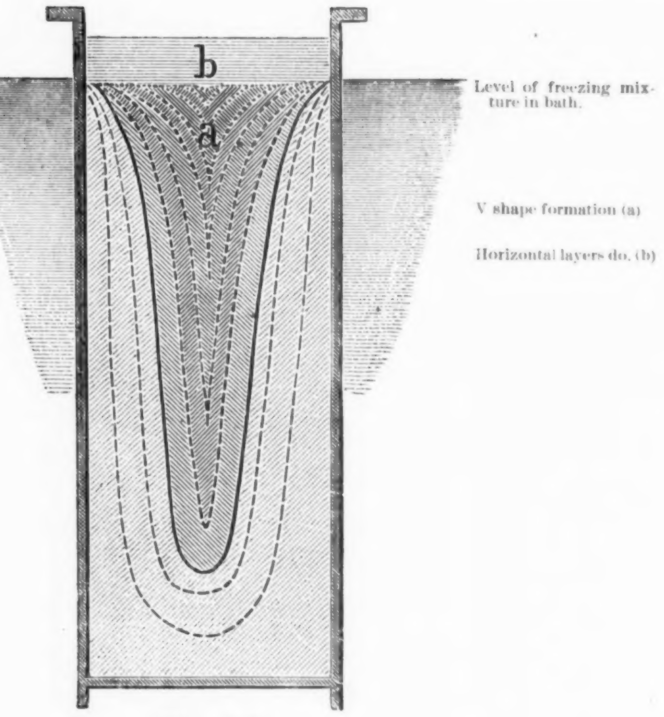
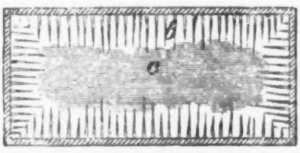


Fig. 66.

Hairbrush Formation.

Fig. 67.



Formation of Ice by Needles on sides.
a. Thin coating on faces of box.
b. Stout thick air needles.
c. Water not frozen.

ipe

t Iron
vanized

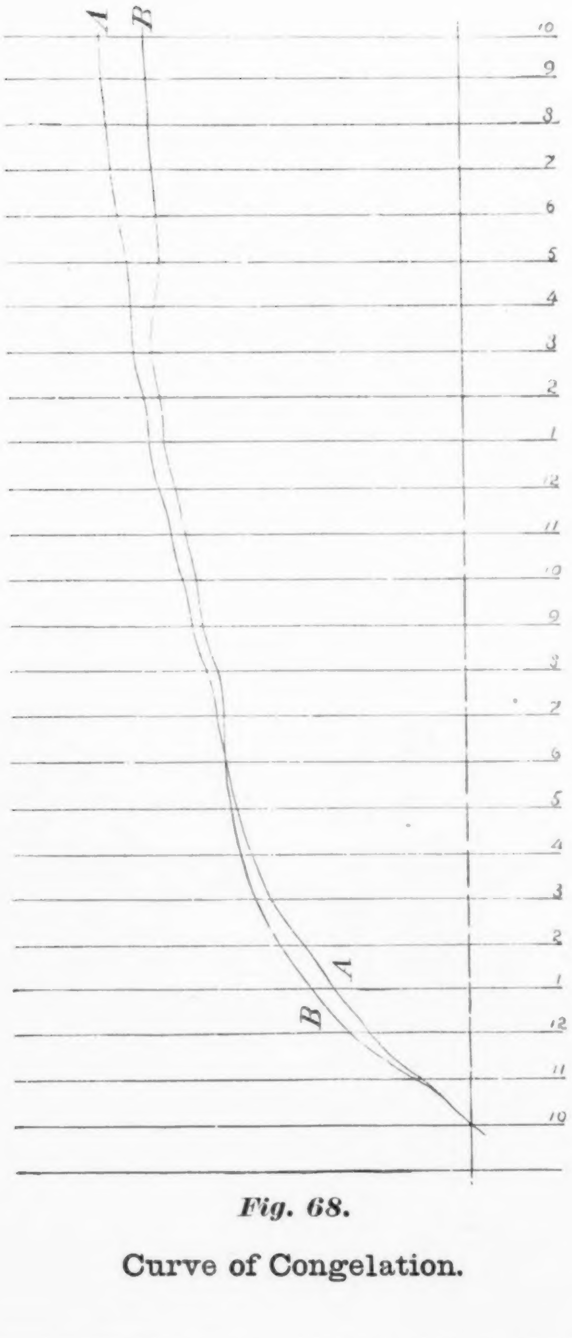


Fig. 68.

Curve of Congelation.

go far to show that the adhesion is as the arc of contact, without reference to the diameter of the pulley.

This matter has been previously ascertained, so far as the different sizes of pulleys are concerned, by the experiments of the late Colonel Morin, and confirmed by those of Messrs. Briggs and Towne, when the arc of contact is 180°; or, in other words, when the belt touched "half-round" the pulley; but I have found no record of actual tests where the belt only touched one-quarter round, or 90°, or where it was carried three-quarters round, or 270°, though I have found plenty of theoretical calculations of what the result ought to be. In order to satisfy myself on this point these experiments were made, but have not yet been carried out as fully as might be wished. They give the additional weight which a belt equally loaded at each end would support on one end without slipping. This result was attained by the use of a model shown in the accompanying engraving (Fig. 69) and loading the belt at

Experiments on Friction of Belts at Different Angles of Contact on Pulleys of Different Diameters and with Different Conditions of Belt.

	Old 3-inch Belt—moist and soft—March 10, 1881.						Same Belt—more dry, April 2, 1881.			New Belt—April 2, 1881.		
	Pulley 36-in. Diam.			Pulley 18-in. Diam.			Pulley 24-in. Diam.			Pulley 24-in. Diam.		
Angle 90°	Weight. Light End.	Weight. Heavy End.	Weight Held.	Weight. Light End.	Weight. Heavy End.	Weight Held.	Weight. Light End.	Weight. Heavy End.	Weight Held.	Weight. Light End.	Weight. Heavy End.	Weight Held.
	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
	21.75	34.	12.25	21.75	34.	12.25	21.75	31.50	9.75	21.75	31.	9.25
	30.25	45.75	15.50	29.75	43.75	14.	31.25	47.25	16.	35.	48.	13.
	45.75	67.75	22.	43.75	66.50	22.75	44.75	65.	20.25	53.	71.	18.
	55.75	84.75	29.	52.3	85.	32.25	52.75	76.	23.25	52.75	70.25	17.50
	72.50	107.25	34.75	70.75	105.25	34.50	69.75	100.75	31.	70.75	92.75	22.
Totals...........	226.00	339.50	113.50	218.75	334.5	115.75	220.25	320.50	100.25	233.25	313.	79.75
Averages = ...	45.20	67.9	22.70	43.75	66.90	23.15	44.05	64.10	20.05	46.65	62.60	15.95
Angle 130°	21.75	37.75	16.									
	29.75	50.25	20.50									
	44.25	74.	29.75									
	22.75	91.	38.25									
	70.25	118.	47.75									
Totals...........	218.75	371.	152.25									
Averages = ...	43.75	74.20	30.45									

Experiments on Friction of Belts at Different Angles of Contact on Pulleys of Different Diameters and with Different Conditions of Belt.

CONTINUED.

	Old 3-inch Belt—moist and soft—March 10, 1881.						Same Belt—more dry, April 2, 1881.			New Belt—April 2, 1881.		
	Pulley 36-in. Diam.			Pulley 18-in. Diam.			Pulley 24-in. Diam.			Pulley 24-in. Diam.		
Angle 180°	Weight. Light End.	Weight. Heavy End.	Weight Held.	Weight. Light End.	Weight. Heavy End.	Weight Held.	Weight. Light End.	Weight. Heavy End.	Weight Held.	Weight. Light End.	Weight. Heavy End.	Weight Held.
	lbs. 25. 35.25 41.75 46.75 70.25	lbs. 53.25 69.25 84.25 93. 134.75	lbs. 28.25 34. 42.5 46.25 64.50	lbs. 21.75 36.25 44.75 52.75 69.75	lbs. 48. 79. 97. 114. 142.25	lbs. 26.25 42.75 52.25 61.25 72.50	lbs. 21.75 30.25 44.75 52.75 69.75	lbs. 44.25 59.25 84.75 99.25 130.25	lbs. 22.50 29. 40. 46.50 60.50	lbs. 21.75 30.25 44.75 52.75 69.75	lbs 37.50 52.25 73.75 85 75 108.75	lbs. 15.75 22. 29. 33. 39.
Totals..........	219.	434.50	215.50	225.25	480.25	255.	219.25	417.75	198.50	219.25	358.	138.75
Averages = ...	43.80	86.90	43.10	45.05	96.05	51.	43.85	83.55	39.70	43.85	71.60	27.75
Angle 220°	23.25 29.75 44.25 52.75 69.75	54.75 71.50 105. 120.25 165.75	31.50 41.75 60.75 67.50 96.									
Totals..........	219.75	517.25	297.50									
Averages = ...	43.95	103.45	59.50									
Angle 270°	21.75 33.50 44.25 62.75	89.75 116.25 147.25 196.25	68. 82.75 103. 133.50									
Totals..........	162.25	549.50	387.23									
Averages = ...	40.56	137.37	96.81									
Angle 270° Same belt as used on 24-in. Pulley.	21.75 30.25 44.75 52.75 63.75 70.75	76. 103.75 138.50 161.25 188.50 205.25	54.25 73.50 93.75 108.50 124.75 134.50									
Totals..........	284.	873.25	589.25									
Averages = ...	47.33	145.54	98.21									

one end till it slipped, and then reducing the load by half a pound at a time until it became stationary.

The belts used were three inches wide, one entirely new, one

old, but in good order and quite soft with oil, and finally the same belt after drying three weeks.

The total weight of belt and weight pans at each end was $14\frac{1}{2}$ pounds, or $7\frac{1}{4}$ pounds for each end.

To obtain the different angles, the belt was led over an anti-friction roller of wood, 3″ in diameter, 6″ long, and hung on a steel spindle, $\frac{1}{4}$″ in diameter, which would move with the difference in weight of the ends of the belt only, if the latter was not carefully balanced over it. This roller was supported in an adjustable stand, which allowed it to be placed so as to cause the belt to subtend different segments of the pulley as shown in the sketch.

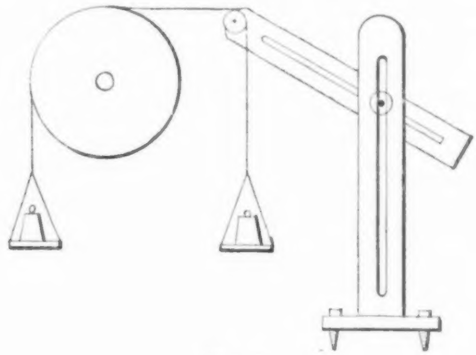


FIG. 69.

The 36″ pulley had been used in a cotton mill, and was highly polished. The 26″ was finished ready for use, and the 18″ was also called "finished," but not so perfectly as the 26″ one.

The results show that with a friction contact of 90° the additional weight supported without slip was about one-fourth of the original load. With 180° contact the weight supported in addition was about one-half the original, and with 270° rather more than the same, or in other words, at 90° equalled one-third the pull on the strained end; at 180° equalled one-half the pull, and at 270° equalled two-thirds the pull.

Other business has prevented me from preparing a paper which would show more fully the practical deductions which may be drawn from these experiments, but I hope that it may be in my power to do so before the next meeting of the Society of Mechanical Engineers.

Meanwhile, these experiments may be of value to others, as they stand, should their completion be further interfered with.

DISCUSSION.

Mr. Nagle: I will state that in Mr. Cooper's book there are some experiments recorded of the same nature as those which Mr. Webber has recorded.

Mr. George Alden: I would like to inquire whether the practical results of the experiment confirm the formula, or otherwise; whether it was found that the formula agreed with the practical results or disagreed largely?

Mr. Nagle: There is a very wide diversity in the results of these experiments. In Mr. Cooper's book on Belting, there are some experiments made in a similar manner to those of Mr. Webber, and those experiments practically confirm the theory. I can pick out isolated cases from these experiments that confirm the theory; others will not. I have not had time enough to examine it as closely as I shall by and by; but I see occasionally a figure that will conform to the theory. You must remember that they are always making so many experiments producing diverging results that it is very difficult to draw a conclusion from any one of them.

XXXVIII.

In Memoriam.

HENRY ROSSITER WORTHINGTON, LATE VICE-PRESIDENT AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

The wide and profound expressions of regret at the sudden decease of Mr. Worthington, among his professional acquaintances and in the great circles of his friends, was first and largely an expression of personal bereavement. He had earned a high place as an ingenious inventor and a successful engineer, and his work will leave an indelible impress upon professional practice; but the influence and the traditions of him as a man and a friend, will outlive generations of engineers.

The foundation of this mingled esteem and affection was his intense and abiding love of the truth; the foundation was built upon by scientific methods, and the structure was adorned by personal graces and accomplishments. The love of truth, that came to him from a high-minded ancestry, was nurtured by his professional pur-

suits—for his profession, unlike some other professions, and this is their misfortune, not their fault, has one inevitable criterion, and that is, the truth. This sentiment—for it grew in him from a conviction to a sentiment—not only controlled his professional and private conduct, but it stimulated in him an honest skepticism regarding those beliefs in general which have come down to us with no higher authority than that they are an inheritance. He was a willing and valiant assailant of "humbug" in every form; and, nobler than this, he was the patient iconoclast who dispelled the phantoms in the mind of many an inventor, and who saved many a plodding experimenter—not in applied science alone—from impending disaster.

But he was also endowed with a grand humanity which practice perfected. Nor were his friends, so called, the sole beneficiaries; only a long and intimate fellowship with him has discovered many of his private charities, and the half of them will probably never be known.

These attributes found apt and eloquent expression in his scholarly culture, and brilliancy in his spontaneous and perennial wit. As the patient, but not generally unimpassioned, advocate of a truth, or as the exposer of a fallacy or an imposture, by analysis, by analogy, by ridicule, he had few equals.

And to crown all, was his overflowing good-fellowship—with all his serious thoughts and moods—his love of humor and mirth, of intimate talks with groups of friends, rambling from grave to gay, when all his true and kind, and withal his fantastic inspirations would grow into bloom. It was an education to hear him talk when the subject was large enough to move him.

He had his weaknesses and his trials, and he probably had the faults as well as the virtues which grew out of them. But another eye must discern and another hand may formulate his serious faults, if such he had. He was often severely critical, and his intense pursuit of error sometimes led him into a general onslaught flavoring of pessimism. He allowed himself to dispute over details, and the unconformable concrete, when all the time he had been formulating the abstract. But he often purposely blunted the edge of his keenest blade—a grotesque overstatement, a ridiculous *non sequitur*, a fantastic similitude—something kindly enough to break the fall of his victim, but wise enough to avert an anticlimax.

The time is not ripe to analyze Mr. Worthington's contributions

to the engineering specialty in which he did not claim, but in which he was assigned, by general consent, the highest place. Mr. Worthington was undoubtedly the first proposer and constructor of the direct steam-pump. The duplex system in pumping-engines—one engine actuating the steam-valves of the other, causing a pause of the pistons at the end of the stroke, so that the water-valves can seat themselves quietly and preserve a uniform water pressure, thus being a vast improvement on the Cornish engine—is generally admitted to be one of the most ingenious and effective, and certainly one of the most largely applied advances in modern engineering.

Mr. Worthington was chiefly known as a hydraulic engineer; but apart from this specialty, his experimental and practical contributions to other departments of engineering, such as canal steam navigation, compound engines, instruments of precision, and machine tools, would entitle him to a high position in the profession.

Mr. Worthington was born, December 17th, 1817, and died, December 7th, 1880. His ancestors in America were sprung from Sir Nicholas Worthington, of Worthington, England, who died at Naseby for King Charles, and they came to America in 1649.

It would be interesting to trace the history of this family, especially of the grand old father, Asa Worthington. A minute review of the life of Henry Rossiter Worthington, with its multitudinous benefactions of invention, of counsel, of entertainment, would also be pleasing and instructive; but this is not the time nor the place.

His mortal remains lie on the edge of the old rocks which geologists call the primal continent; and every following cycle furnishes some stone to lay on his grave. So his immortal remains illustrate every phase of progress, from the Silurian instinct to live, to the last formula of civilization—to let live.

PROCEEDINGS

OF THE

ALTOONA MEETING, 1881.

XXXIX.

PROCEEDINGS

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

SECOND REGULAR MEETING OF 1881.

ALTOONA, PENNSYLVANIA.

THE Second Regular Meeting of 1881 of the American Society of Mechanical Engineers was held at the Opera House, Altoona, Pa., August 10th–12th.

The following members and guests were registered:

J. C. Bayles,	New York City.
George M. Bond,	Hartford, Conn.
James Brady,	Brooklyn, N. Y.
A. H. Campbell,	Hartford, Conn.
A. C. Christensen,	Brooklyn, N. Y.
John W. Cloud,	Altoona, Pa.
W. B. Cogswell,	Syracuse, N. Y.
W. F. Durfee,	Bridgeport, Conn.
Professor Thomas Egleston,	New York City.
Howard Fry,	Williamsport, Pa.
J. L. Gill, Jr.,	Pittsburgh, Pa.
Frederick W. Gordon,	Pittsburgh, Pa.
Robert Grimshaw,	Philadelphia, Pa.
H. Hamilton,	Youngstown, Ohio.
H. S. Hayward,	Jersey City, N. J.
J. F. Holloway,	Cleveland, Ohio.
L. G. Laureau,	New York City.
W. Barnet Le Van,	Philadelphia, Pa.
Lewis F. Lyne,	New York City.
Joseph Morgan, Jr.,	Johnstown, Pa.
C. C. Newton,	Philadelphia, Pa.
W. E. Partridge,	New York City.
Franklin Phillips,	Newark, N. J.
Charles T. Porter,	Philadelphia, Pa.

Thomas Whiteside Rae,	New York City.
Jacob Reese,	Pittsburgh, Pa.
Professor S. W. Robinson,	Columbus, Ohio.
Oberlin Smith,	Bridgeton, N. J.
Allan Stirling,	Drifton, Pa.
Ambrose Swasey,	Cleveland, Ohio.
John E. Sweet,	Syracuse, N. Y.
Professor R. H. Thurston,	Hoboken, N. J.
Edward N. Trump,	Wilmington, Del.
A. S. Vogt,	Altoona, Pa.
S. B. Whiting,	Pottsville, Pa.
Walter Wood,	Philadelphia, Pa.
C. J. H. Woodbury,	Boston, Mass.

The meeting was called to order at 10 A.M., August 10th, by the President, who addressed the Society as follows:

In opening this second meeting of 1881, I have great pleasure in saying that the Society is fully as prosperous as when the last meeting was held. We have had twenty-four additions to our roll of members, and our Treasurer's report will be found to be quite as satisfactory as the Secretary's. The meeting was called at Altoona, after discussion by the Council of the advisability of calling it here or at Boston. Special requests were made that the Council take into consideration Boston as a place of meeting this season; and, on the other hand, there were one or two protests made against our going there by Boston men. It was urged that people were out of town, and consequently the Society could not receive the attention at this season that it might receive at some spring meeting; and it was also said that the preparations going on there for the Fair to be held in the autumn would prevent as much attention being paid to us as we would probably like. So that on the whole it was considered by the Council best to appoint the meeting at Altoona. You will find here at Altoona a great deal to interest you. Here is a town of twelve or fifteen thousand inhabitants, which has grown up around the shops of the Pennsylvania Railroad Company, and is supported almost entirely by the Pennsylvania Railroad's shops and adjuncts. The shops will be found as interesting, probably, as any establishments of the kind in the world; and our programme, as you see, includes the spending of this afternoon among those shops. The Secretary has a programme giving more of detail, which he will distribute to the members, and he will give instructions, where you desire personal instructions, as to the methods of reaching the different shops and of finding points of interest. The general programme of the

meeting, I presume, members have all received. It announces the opening session this morning, and a visit this afternoon to the Pennsylvania Railroad's shops, and this evening, a session at eight o'clock. It was proposed at first that we should have a dinner this evening for such of the members as chose to be present, but for reasons that the Secretary considered to be sufficient it was finally determined to postpone the dinner until to-morrow, devoting this evening to a regular session. To-morrow we spend the day at the Cambria Iron Works at Johnstown, where you will find enough to interest you for more than the full period of time allowed. To-morrow evening we have our dinner for those who desire to attend, and on Friday a session in the morning and afternoon, adjourning in time to take the evening trains.

The first business this morning is the reading of the minutes of the last meeting.

The Secretary then read the minutes of the last meeting, which, on motion, were approved as read.

The President: The most important business that comes before the Society at this meeting is the appointment of a Nominating Committee to nominate officers for the coming year. The rules provide in regard to this matter, and the Nominating Committee will have to present to the Society the names of a president, three vice-presidents, three managers, and a treasurer.

It is proper then, gentlemen, to provide for the appointment of that committee. How will you have it appointed?

It was moved and seconded that the committee be appointed by the chair.

The motion was agreed to.

Mr. Durfee: I move that the rules be suspended in order to allow the resolution, which I introduced at the last meeting, to be put on its passage at the present one.

Professor Egleston: I second the motion.

Mr. Durfee's resolution was, at his request, then read by the Secretary.

The Secretary: I would like to say, Mr. President, that other amendments kindred to that of Mr. Durfee will be presented at this meeting, and I think that before the vote is taken, it might be well that they should be introduced.

Mr. Durfee: As I understand it, Mr. President, the motion that I made is hardly a debatable one. It does not affect the resolution itself. It only touches the question whether we will

suspend our rules in order to put the proposed amendment on its passage.

THE PRESIDENT: The gentlemen may discuss this question as much as they choose—the advisability of a suspension of the rules. The rule as to amendment reads: "These rules may be amended at any annual meeting by a two-thirds vote of the members present, provided that written notice of the proposed amendment shall have been given at a previous meeting."

That is the rule which it is proposed to suspend. It is for the Society to decide whether the rule shall be suspended or not.

MR. DURFEE: We have a precedent for the suspension of the rules. At the last regular meeting a resolution was introduced by Mr. Holley, which was put on its passage under a suspension of the rules.

The motion to suspend the rules was agreed to.

MR. BAYLES: As Mr. Durfee's motion is before the meeting for consideration, I should like to offer a substitute for that amendment, as follows:

Article XXXVIII to read:

"The Council shall have power to decide on the propriety of communicating to the Society any paper which may be received, or to refer it back to its author for revision or amendment; also, to decide which of the papers read before the Society shall be printed in the *Transactions*. Before such paper appears in the *Transactions* of the Society, a revised proof of the paper and discussion shall be sent by the Secretary to the author, and so far as practicable to every member taking part in the discussion, with request that they call attention to any errors therein. When the Council shall so direct, printed copies of papers shall be distributed to the membership in advance of the meeting at which they are to be presented and discussed."

MR. DURFEE: I think the amendment as proposed by Mr. Bayles is perhaps a little more comprehensive than my own, and covers the entire ground that I intended to cover. I have no objection to accept it as a substitute.

THE PRESIDENT: Does Mr. Durfee's seconder accept?

Professor Egleston signified his acceptance of the substitute.

DR. GRIMSHAW: I think that any paper which is worthy of being read before the Society, is worthy of being printed in the *Transactions*; and our experience with those papers printed beforehand for distribution among the members, warrants us I think, in believing that this is one of the most essential matters. It enables us to discuss papers so much more intelligently. It saves the time of asking questions where members do not understand.

For that reason I would ask Mr. Bayles if he would amend his amendment by making it so that all papers should be printed and handed around before they are read.

MR. BAYLES: I hope the gentleman will not press that at this time, because there is another amendment to follow which will cover that. As it stands now, it says that when the Council shall direct, this shall be done. This matter should be left in some degree discretionary with the Council.

The amendment to Article XXXVIII was adopted; Mr. Rae, however, voting Nay.

MR. BAYLES: Under the suspension of the rules, I should like to offer an amendment to Article XL, which as amended would read:

"The Society shall claim no exclusive copyright in papers read at its meetings or in reports of discussions thereon, except in the matter of official publication with the Society's imprint, as its *Transactions*. The Secretary shall have sole possession of papers between the time of their acceptance by the Council and their reading, together with the drawings illustrating the same; and at the time of said reading he shall have printed copies for distribution to members present, and shall give the same to representatives of such newspapers as desire them for unofficial publication, in whole or in part. Copies of the drawings shall at the same time be furnished to journals which have previously made application for them, at the cost of making such copies. *Provided*, That the author of a paper shall in no case be deprived of his right to give copies of the same to any one he chooses, before it is read or afterwards; but if such paper is published unofficially prior to the meeting at which it is to be read, it shall be considered as withdrawn by the author, and shall not be presented for reading or discussion as a paper of the Society."

THE PRESIDENT: This, I understand, is a new amendment. It simply can be read and must be laid over till the next meeting.

MR. BAYLES: I offer it under the suspension of the rules with the request that it be put on its merits.

THE PRESIDENT: It was not the understanding of the chair that the suspension of the rules permitted anything more than the passage of the amendment already offered, which was offered at a previous meeting. This amendment would properly come up at the next meeting. Of course if the gentlemen choose they may suspend the rules and take it up now. It is simply a question of propriety to be settled by the members present. If such a motion is made it will be put, of course. The vote already taken does not include such a suspension of the rules as would permit this amendment now offered to be voted upon. Have you such a motion to make, Mr. Bayles?

Mr. Bayles: I am uncertain about the expediency of making such a motion, Mr. President.

The President: If no such motion is made, it will go over, under the rules, to the next meeting.

Mr. Rae: I desire to explain my negative vote and to call the attention of the Society to one feature of this amendment to Article XXXVIII, which only struck me rather late in the day. It assumes that the Secretary is really responsible, *ex officio*, for all the acts of the Publication Committee. The Secretary happens to be a member of the Publication Committee, and is its active member, and a liberal construction of this amendment fits the present case exactly. When we are doing so important a thing as amending our rules we should be very careful. This does not fall upon the Secretary as secretary, but simply as a member of the Publication Committee. Would it be advisable to modify that amendment so as to read the Publication Committee, instead of the Secretary?

Mr. Bayles: So far as I can see, sir, the only mention of the Secretary in Article XXXVIII is that he shall send copies to the persons interested. I think that is properly a secretary's duty.

The President: The amendment makes it the duty of the Secretary.

Mr. Rae: If it be understood that the Secretary is *ex officio* a member of the Publication Committee, it would be perfectly logical.

Mr. Bayles: To save time I will move to suspend the rules and bring up this amendment to Article XL on its merits.

Professor Egleston: One of the greatest difficulties in starting any society is the temptation to occupy its time with a great deal of legislation; the whole country is burdened with legislation, it is the mania of the age. Many a young society has been stifled in its organization by its ponderous Constitution and By-Laws. If this society commences at this early stage with amendments to its Constitution and By-Laws—which are already good enough—the quicker the Society gets through with it and goes to active work the better. I therefore suggested to Mr. Bayles to have all the amendments voted on now, and then devote the time to engineering subjects, which are the proper province of this society. It is a great pity to have the time of those who are so occupied as most of the members of this society are, and who come here from a distance at a considerable sacrifice, taken up with the discussion of trivial matters.

A division having been taken, five voted in the affirmative and six in the negative, and the motion to suspend the rules was declared lost.

Mr. Bayles: I give notice that I offer this as an amendment and will bring it up at the next meeting.

The Secretary then read Mr. Reese's paper, entitled "Rolled Cast-Steel Car-Wheels."

Professor Thurston read a paper entitled, "A Note on the Proper Method of Expansion and Regulation of the Steam-engine," Mr. Porter taking the chair.

Evening Session (August 10th).

The President called the meeting to order at 8.30 p.m., and stated that a Council meeting would take place at ten o'clock, immediately after the adjournment.

A report from the Treasurer was presented by Mr. Rae.

The President: If there is no objection the report will stand approved.

A paper by Mr. Hague, entitled "Comparisons between Different Types of Engines," was read by the Secretary. No discussion followed.

Mr. Rae read a paper on "The Latest Methods of Submarine Telegraph Work."

Professor Sweet read a paper entitled "Coffin's Averaging Instrument."

After some discussion, which, on motion of Mr. Rae, it was agreed should be erased from the minutes, Mr. Bayles moved that Mr. Le Van and Dr. Grimshaw should be added to the Committee on Regular Meetings, which was agreed to.

SESSION OF AUGUST 12th.

The meeting was called to order at 10 a.m.

The Secretary read letters from the Secretary of the American Society of Civil Engineers, the Chief Engineer of the U. S. Army, and the Secretary of the United States Naval Institute.

Professor Robinson then read a paper entitled "Counterbalancing of Engines and Other Machinery having Reciprocating Parts."

AFTERNOON SESSION (AUGUST 12TH).

Before calling the meeting to order, the President requested members to examine designs for a diploma to be issued to its members by the Society, which were lying on the Secretary's table.

The President also stated that the Council would be pleased if the members would suggest designs for a seal for the Society.

With respect to the proposed incorporation of the Society, the President said:

I presume that we all consider it desirable to make the Society an incorporated body as early as possible. Gentlemen who contemplate joining the Society will always feel a little easier if it is an incorporated body, although it is not likely that the debts of the Society will ever be serious in amount; but the fact that it is an incorporated Society will give it a standing that it would not otherwise have. The Council would like the members to discuss the matter freely.

Resolutions of thanks were then read by Mr. Porter and adopted unanimously.

Mr. Oberlin Smith read a paper entitled "The Nomenclature of Machine Details."

Mr. Hall's paper entitled "A Method of Arranging and Indexing Drawings and Patterns," was then read by Mr. Woodbury.

The Secretary then read a paper by Messrs. Wolff and Denton entitled "The Most Economical Point of Cut-off in Steam-Engines."

THE PRESIDENT: There is but one paper remaining, and that, it is requested, shall be read by title; and before closing the business of the afternoon, I have to announce simply that we shall meet in New York, on the first Thursday of November next. The hour will be given in the call—the same as last year, I presume. The paper to be read by title is by Mr. Woodbury on the "Fire Protection of Mills." If there is no business to be presented a motion to adjourn will be in order.

On motion, the meeting was adjourned.

PAPERS

OF THE

ALTOONA MEETING, 1881.

XL.

COUNTER-BALANCING OF ENGINES AND OTHER MACHINERY HAVING RECIPROCATING PARTS.

BY S. W. ROBINSON, PROF. MECH. ENG., STATE UNIVERSITY, COLUMBUS, OHIO.

THIS subject is an important one, and seems to be none too well understood. Some appear to imagine that a reciprocating piece can be perfectly counter-balanced by a revolving piece, provided the relation of the weights is right. In making a practical search for the right weight, by putting on one and then another, they find none to suit exactly, and wonder what is the matter. The short of it is, *it is simply impossible* for a *single* reciprocating piece to be perfectly counter-balanced by a rotating piece or pieces revolving one way. It is possible, however, perfectly to counter-balance two reciprocating pieces, like two pistons of equal weight, actuated from one crank-pin, and sliding in straight lines at right angles to each other, the lines intersecting at the crank-shaft, and this by one counter-weight. The connecting-rods in this case are supposed without weight, and not to have sensible obliquity at mid-stroke. When the connecting-rod is five or eight times the length of crank, the counter-balancing can be made nearly perfect, but not quite. In this case of two pistons, if the counter-weight can have its centre of gravity at the same distance from the shaft as the crank-pin is, its weight should be equal to that of one piston-rod and cross-head, the connecting-rod being supposed without weight.

But let us take some of the simpler problems first.

The effect of a counter-balance should be considered with regard to the effect of its centrifugal force. This may be found by the following

RULES FOR CENTRIFUGAL FORCE.

This force, in pounds, is equal to one thirty-second part* of the weight of the rotating mass, in pounds, multiplied by the square of the velocity, in feet per second, with which the centre of gravity of the weight moves in its path, and divided by the radius, in feet, of the circle of motion of the centre of gravity of the weight.

---

* More exactly $= \frac{1}{g} = \frac{1}{32.\frac{1}{6}}$.

Or again, it equals 1.227 times the weight of the revolving mass, in pounds, multiplied by the square of the number of revolutions per second, and multiplied by the radius, in feet, of the circle of motion of the centre of gravity of the weight.

EXAMPLES.

As an example, suppose a scythe snath, weighing 10 pounds, be placed inside the rim of a large pulley and tied so as to hold it in position. Suppose the centre of gravity of the snath be five-ninths of its length from the tip-end, and four inches from the body of the snath. When tied in, suppose the centre of gravity be found, by measurement, to be four feet from the axis of the pulley. If this distance is four feet it is immaterial what position the snath may have. Then suppose snath and pulley be set to revolving at 100 revolutions per second. The centrifugal force F will be by the second rule,

$$F = 1.227 \times 10 \text{ lbs.} \times (100 \text{ rev.})^2 \times 4 \text{ ft. radius} = 490,800 \text{ lbs.} = 245 \text{ tons.}$$

A very useful point, in calculation of centrifugal force, is that of regarding solely the centre of gravity of the weight to which the centrifugal force is due. It enables us to dispose of all irregular bodies by supposing the whole mass concentrated at the centre of gravity, the calculation of the centrifugal force of which is then easily made.

The subjoined examples follow as consequences:

1°. Suppose a balanced pulley A, Fig. 70, with three weights riveted upon it, as at C, D, and E. The centre of gravity of each is shown by a dot. We usually suppose that if the system is in balance when the axis of the pulley rests on knife-edges, that it will run without shake. This is true if the centres of gravity are all in one plane, perpendicular to the axis of pulley, but otherwise not. This fact would do possibly for a proof of the above principle of the reduction of mass to its centre of gravity. According to that, suppose that

$$EAD, = 90° \text{ and } CAE, = CAD, = 180° - 45°.$$

The centre of gravity of the two weights, E and D, is evidently at B, if the two weights are equal. Then the added weights of E and D must be to that of C, as CA is to AB.

By calculation,

$$\frac{AB}{AC} = 0.7071.$$

Hence,

$$\frac{\text{weight of } C}{\text{weight of } (E+D)} = 0.7071 = \frac{AB}{AC}.$$

From the rule it evidently does not matter where the weights E and D are placed along the line ED produced, provided they are at equal distances from B.

Again, it is immaterial what position the weight C has, provided its centre of gravity remains at C. That is, any position shown by dotted lines in Fig. 71 may be taken. At first this

FIG. 70.

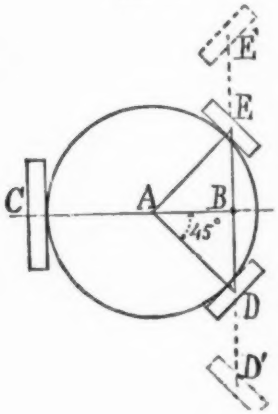
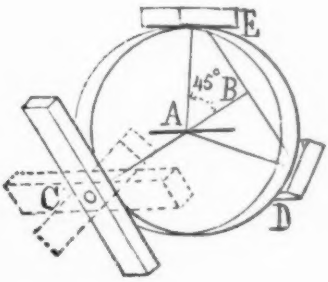
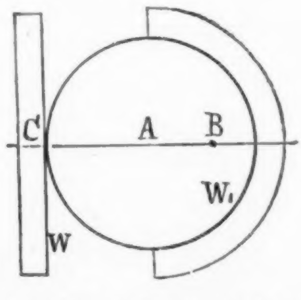


FIG. 71.



seems untrue, because the ends of the elongated weight C for one position are much further from the axis of revolution than for that position where it is parallel to the axis. But the radius to the centre of gravity is constant.

FIG. 72.



2°. Suppose weights W and W_1 attached to the pulley as shown in Fig. 72. Let the thickness of the two weights be the same, and

attached to the true rim of the pulley by rivets or flush-screws, so that it will not be necessary to take screw-heads into account.

Then, if the thickness of the weights is one-tenth the radius of the pulley the distances to the centres of gravity will be, as found by calculation:

$$AB = 0.669 \times \text{radius of pulley},$$
$$AC = 1.05 \times \text{radius of pulley},$$

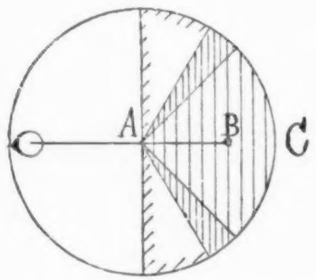
and hence

$$\frac{W}{W_1} = .6371 = \frac{AB}{AC}$$

Hence, if $W_1 = 100$ lbs., $W = 63.71$ lbs.

3°. Suppose three cases of counter-weight of an engine or jig-saw in form of a sector of a circle, first, of 180°; second, of 120°;

FIG. 73.



and third, of 90°, and of uniform thickness, all having the same radius as shown in Fig. 73.

Then, by calculation:

$$AB = \frac{4}{3\pi} AC \text{ for sector} = 180°,$$

$$AB = \frac{4\sqrt{3}}{3\pi} AC \text{ for sector} = 120°,$$

$$AB = \frac{4\sqrt{2}}{3\pi} AC \text{ for sector} = 90°,$$

and

$$\frac{\text{centrifugal force for } 120°}{\text{centrifugal force for } 180°} = .866,$$
$$\frac{\text{centrifugal force for } 90°}{\text{centrifugal force for } 180°} = .707.$$

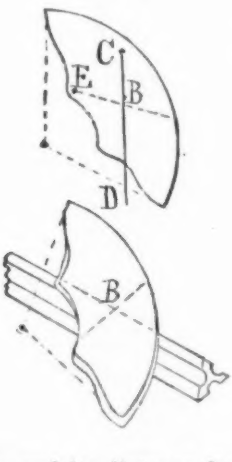
That is, if we add fifty per cent. to the sector of 120° by extending it to 180°, with the same thickness, we increase the centrifugal force only about a seventh. Again, if the sector of 90° be ex-

tended similarly to 180°, or the weight doubled, the centrifugal force will be intensified only about a third. These figures show the comparatively small value of parts of a counter-balance which are nearly opposite each other.

PRACTICAL ESTIMATION OF COUNTER-BALANCE WEIGHTS.

In designing counter-balance weights which are of uniform thickness in the manner of the weights in Fig. 73, a convenient way to proceed is to cut out a piece of cardboard of the shape of the weight, as shown in Fig. 74, find its centre of gravity by hanging it upon a pin *C*, so that it will swing freely, then note a vertical line through the pin by means of the plumb-line *CD*. Then similarly find a second line, *EB*, intersecting the first at *B*. Then *B*

FIG. 74.



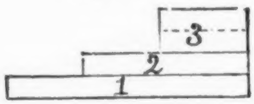
is the centre of gravity, and its distance from the axis of rotation is readily found when the weight is mounted in position. If the weight of the piece is known, the centrifugal force can be calculated easily. The centre of gravity is, perhaps, most conveniently obtained by balancing the cardboard weight on a knife-edge, and noting the line for two positions, as shown in the second part of Fig. 74.

The actual weight of the counter-balance, Fig. 74, however awkward in outline, may be found by cutting a square from the same piece of cardboard from which the above shape, Fig. 74, was cut, and large enough to represent a square foot on the same scale by which Fig. 74 card was cut. For instance, if Fig. 74 is cut "half

size," the square piece would be half a foot, or six inches square. Now weigh the two pieces of card separately. Then if the Fig. 74 piece weighs two-thirds as much as the square piece, the actual iron weight, Fig. 74, one inch thick, will weigh two-thirds as much as a piece of iron one foot square and one inch thick.

When the counter-weight has several thicknesses, we may proceed similarly. For instance, suppose one part of the weight, Fig. 74, has a certain thickness, another part twice as thick, and a third part four times as thick, as shown in Fig. 75. That is, the slices Nos. 1 and 2 are of equal thickness, while the part No. 3 is twice as thick as 1 or 2, as stated. Then cut one piece of card for No. 1, one piece for No. 2, and two pieces for No. 3. Then pile them, and stick with a little gum, so that we have the counter-balance in miniature. Then find the centre of gravity as before. Also weigh it, and weigh a piece of the same card cut to the same scale and representing one square foot. Then the actual counter-balance weight, Fig. 75, four inches thick, will weigh as many times the weight of a piece of iron one foot square and one inch thick, as

FIG. 75.



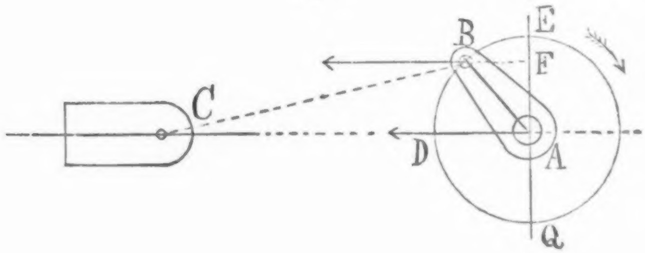
the cardboard weight, Fig. 75, is times heavier than the card representing one square foot. Or, if a planimeter is at hand, the areas of the several pieces may be obtained before piling. Of course the relation of the areas will be the same as that of the weights of cardboard, or of the iron. Knowing the weight of the counter-balance and the position of its centre of gravity, not only the amount of its centrifugal force, but also the direction of it will be readily found, the direction being that of a line passing through the axis of rotation and of the centre of gravity.

ACTION OF RECIPROCATING PARTS UPON A CRANK-PIN.

Let us next inquire into the action of a reciprocating piece, like a piston, upon the crank-pin by which it is actuated. First, consider the connecting-rod as without weight, and as causing no sensible modifying action, on account of its obliquity at mid-stroke. Also, suppose the crank-pin to move with uniform velocity in its orbit, as when a fly-wheel is mounted on its crank-shaft.

Then for the position shown in Fig. 76, the piston is being dragged toward A, and the force exerted against the crank-pin will be nearly parallel to the line AC, this force being caused by the acceleration the piston is undergoing. For the crank-pin at E, the piston will have attained the speed of the crank-pin in its path, and the acceleration will be zero. At D the acceleration, or rate of increase of speed of piston, will be the greatest; and hence, at this point, the pressure against the crank-pin is at its maximum. The pressure of steam on the piston in expansion, cushion, etc., is without account in causing shake, because the same pressure which is being exerted on the piston has an equal reaction upon the cylinder-heads. The shake of the engine, jig-saw, etc., is a vibratory motion of the frame of the machine, and is due to forces acting upon the frame. As regards a line through the shaft and cylinder, it matters not whether the whole force acts upon the pil-

Fig. 76.



low-block, as in absence of steam, or partly there and partly on cylinder-head, in presence of steam.

A rule determined by aid of the higher mathematics, for the pressure upon the crank-pin, due to the acceleration of the piston, is to the effect that at D the pressure is the same as would be the centrifugal force of the reciprocating parts C, were they hung upon the crank-pin and revolving with it, the centre of gravity of C being placed at the crank-pin centre. Also at other points B, this pressure diminishes in proportion to the cosine of the angle BAD. A simple diagram represents this pressure for any point in the revolution. For instance, let AD represent it for the pin at D. Then, BF being the cosine of BAD will represent it for the pin at B, both in magnitude and direction.

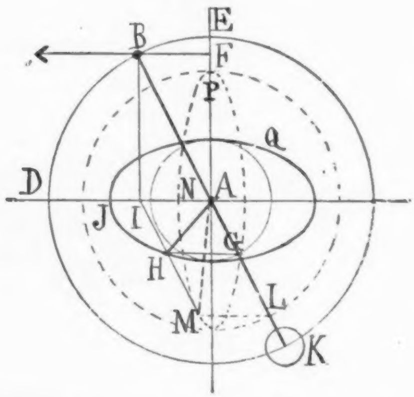
As regards shake, the point of application of this force is at A, it being transmitted from B to A along the crank. It tends to pull the engine in the direction A toward D, while the crank-pin

is in the half circle QDE. But it is opposite for the next half circle, etc. Therefore, though a horizontal line from any points, B to F, represents the direction and magnitude of the pressure tending to cause shake, its point of application is always at A, and hence the shaking force is completely determined.

COUNTER-BALANCING OF RECIPROCATING PARTS.

Now let us consider counter-balancing the weight C, by a revolving piece, the connecting rod still being neglected. Suppose a counter-weight at K, Fig. 77, diametrically opposite the crank-pin B. Let the centrifugal force of the weight K be indicated on the diagram by AG, always toward K. The diagram of centrifugal force

FIG. 77.

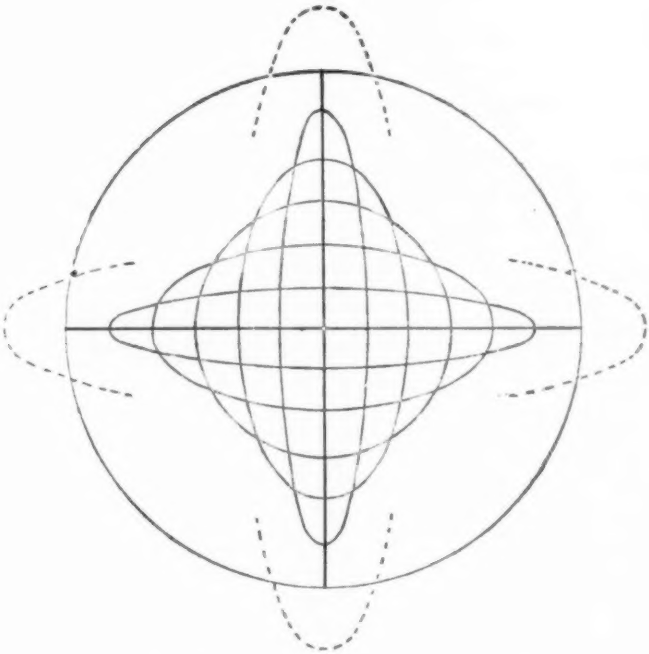


for the revolution will be the circle to the centre A, and radius AG, as shown. Also in the same diagram, let the pressure due to the reciprocating piston, etc., be represented by AD, for the crank-pin at D. Then, as shown above, BF will stand for the pressure upon the pin when at B, though acting on the engine-frame at A. Make $AI = BF$; then for the crank-pin at B, and the counter-weight at K, we have the two forces AI and AG, acting at A, the effect of which is their resultant. Finding this by means of the parallelogram $AIHG$, we get AH as the force, in magnitude and direction, and active at A, which represents the combined effect of the reciprocating parts and counter-weight. Any number of points, H, can easily be found. A curve, traced through all the points H, for a revolution, will be an ellipse HJQ, as shown, the half leng h of which will be $AJ = AD - AG$, and the half width will be AG.

But if the weight K were greater, so as to cause a centrifugal force AL, the resultant of AL and AI will give a point M, in another ellipse MNP, the half width AN being equal $AD - AL$; and the half length being AL.

Now, when the piston remains the same, each counter-balance weight K will have its own ellipse. Thus, K might be given a series of values from zero up, and the ellipses drawn. When K is small, the ellipses will be longest in the direction of AD; and

FIG. 78.



when it is large the ellipses will be longest perpendicularly to AD. Between these limits the ellipse becomes a circle, the radius of which is half the radius AD, as in Fig. 78. When $K = 0$ the ellipse becomes a line, viz., the horizontal diameter of the circle of the crank orbit, shown. But when K equals the weight of the reciprocating parts the ellipse becomes the vertical diameter of the circle, Fig. 78.

By varying the weight of K, from zero up to equal the reciprocating parts, a series of ellipses, such as shown in Fig. 78, is obtained. It is interesting to note that if K is negative, that is, acts with

instead of against, the reciprocating parts, we have ellipses over-reaching the crank circle horizontally. Again, if K is greater than the reciprocating parts, vertically overreaching ellipses are obtained. These ellipses are indicated in Fig. 78, in dotted lines, and are probably of little or no service in practice.

If the resultant jerking force be represented by a line like AH, Fig. 77, it will revolve around A in the same direction as does the crank for the case of any dotted overreaching ellipse, Fig. 78. But when the ellipse is wholly within the circle, as the full-line ellipses, Figs. 77 and 78, we notice the extraordinary peculiarity that the resultant force AH, Fig. 77, revolves around A in the opposite direction to that of the crank. Hence the shake, when K equals half the reciprocating parts, is the same as though everything were removed from the crank, and a weight equal half that of the reciprocating parts centred upon the crank-pin, and the crank set to revolving backwards.

As regards the intensity of shake generally, it is due to the resultant AH, acting at A, revolving backwards, and varying in length according to the proper ellipse. This shake is evidently a minimum when the ellipse becomes a circle, half as large as the crank circle, because the greatest value of the resultant for a revolution is here the least.

From these facts, it appears to be impossible to perfectly counterbalance a reciprocating piece by a single rotating piece.

Next suppose there are two pistons, etc., of equal weight, actuated by one crank-pin, the pistons reciprocating in lines AD and AE, Fig. 77, passing through the crank-shaft, and at right angles to each other. Then BF, is the pressure upon the crank-pin for one, and BI, the pressure for the other; both, of course, active at A. Then the line of the crank, AB, is their resultant, in magnitude, direction, and point of application; and it is constant for the entire revolution. It is evident, since the centrifugal force due to a certain weight, K, is constant, that a weight, K, may be so chosen that its centrifugal force shall just equal the resultant AB, and we have a perfect balance. When the radius, AK, equals AB, the weight of the counter-balance, K, must equal that of one piston, piston-rod, and cross-head, connecting-rod being neglected, and in the reverse proportion for other radial distances.

So far, the weight of the connecting-rod has been excluded, and the force upon the crank-pin, due to the reciprocating parts,

has been simply in a line parallel to the reciprocation; the obliquity of the connecting rod having been neglected also.

THE CONNECTING-ROD.

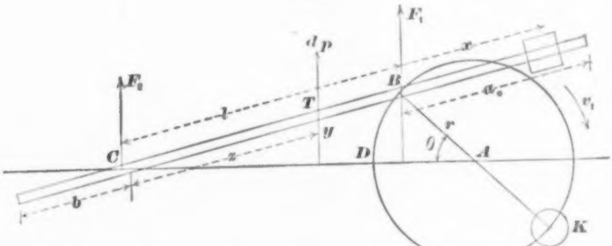
This case is peculiar, because one end swings or rotates around with the crank-pin, while the other slides or reciprocates with the cross-head. The forces made active upon the crank-pin will, therefore, be here due to both reciprocation and rotation. It is evident at once that the resultant action upon the crank-pin, for different points in the revolution, will be variable, the greatest intensity being in the line of reciprocation of cross-head. The least intensity will be nearly in a line at right angles to this, and, though least for a revolution, it will still be considerable, and too great to neglect.

Also, there will be lateral forces made active upon the cross-head pin, sufficient in high-speed engines to throw the cross-head aslam across the backlash in its slides.

We, therefore, have three forces to consider, viz.: first, longitudinal, or in direction of reciprocation of cross-head; second, lateral, both upon the crank-pin; and third, lateral upon the cross-head pin. The intensity of these two last forces depends upon the construction or form of connecting-rod. The first two forces may be considered as components of a resultant force.

The first force is evidently nearly the same as if the mass of the connecting-rod were all concentrated at C, Fig. 76; and the same rules and principles applied to the reciprocating force, C, apply here. That is, at D the force against the crank-pin is equal

FIG. 79.



to the centrifugal force of the mass of the connecting-rod, if hung by its centre of gravity upon the crank-pin; and it varies nearly as the cosine of the crank angle, etc.

The second, or lateral component of force upon crank-pin, can only be found by a somewhat extended investigation, such as finding the lateral acceleration of every element of the connecting-rod, and referring the effect of all to the crank-pin. It will distract too much from the present object to stop here and find this. It is enough to give the results in the following formulas for Fig. 79, where AB is the crank; C the cross-head pin; CB the connecting-rod, assumed of prismatic form, overhanging at each end, and with a weight, w_1, at one end. Then, the lateral force, in a direction at right angles to AC, at B is

$$F_1 = \frac{v_1^2 \sin\theta}{grl^2}\left\{\frac{W}{3}\left(\frac{(l+a)^3+b^3}{l+a+b}\right) + w_1(l+x)^2\right\} \quad . \quad . \quad . \quad (1)$$

and at C is

$$F_2 = \frac{v_1^2 \sin\theta}{grl^2}\left\{\frac{Wl}{2}\left(\frac{(l+a)^2-b^2}{l+a-b}\right) - \frac{W}{3}\left(\frac{(l+a)^3+b^3}{l+a+b}\right) - w_1x(l+x)\right\} \quad . \quad (2)$$

in which

W = weight of the pitman and its overhanging parts a and b.
w_1 = weight attached at the distance x.
v_1 = velocity of crank pin in its orbit, feet per second.
θ = crank angle, reckoned from D.
r = radius AB of crank.
g = acceleration of gravity = $32\frac{1}{6}$ feet per second.
l = length of pitman proper, BC.
a = overhang at crank-pin.
b = overhang at cross-head pin.

NOTE.—To show how to obtain equations (1) and (2), we have from Mechanics, for acceleration or retardation, the fundamental equation:

$$f = \frac{F}{M} = \frac{d^2y}{dt^2} = \frac{dp}{dm} \text{ or } dp = -\frac{d^2y}{dt^2}dm.$$

This is applicable to the elementary mass dm, at T, Fig. 79, of length dz and weight $wdz = gdm$: where w = wt. per unit length of pitman rod. The equation gives the acceleration in a direction perpendicular to AC when the component of motion of mass in this direction only is taken. This acceleration is seen to be negative.

But with reference to the crank B, taking moments about C, we have $dF_1 = \frac{z}{l}dp$. The crankpin velocity at B is $v_1dt = rd\theta$.

Also we have $r \sin\theta = y_1 = \frac{l}{z}y$; whence, $d^2y = -\frac{z}{l}r \sin\theta d\theta^2$.

Substituting d^2y, dt and dm in dp, and dp in dz, we have

$$dF_1 = \frac{wv_1^2 \sin\theta}{grl^2} z^2 dz.$$

Integrating between the limits $l + a$ and $- b$, and putting

$$W = w(l + a + b) = \text{weight of whole rod},$$

we get all of equation (1) except the term containing w_1. That is obtained in a similar manner by aid of the same equation for acceleration where $mg = w$.

The difference in procedure for equation (2) consists in taking moments for dp, w, and F_2 about B. The integral is then taken for the same limits.

For any particular case of a pitman, all the quantities in these equations are constant except θ, and hence these forces, F_1 and F_2, vary as the sines of the crank angles θ: just the same law for the lateral forces as we found for the longitudinal, except the sine is used in place of the cosine; and hence when found for one position, a diagram of sines can be drawn to give the force for any position. That is, if the lateral force at E, Fig. 77, be AE, then the lateral force at B will be IB, and the maximum for F_1 and F_2 is for the crank-pin at E.

1°. If the overhanging parts a and $b = o$, and also $w_1 = o$, we have nearly the ordinary case of a pitman when, for the point E, Fig. 77, the maximum lateral forces for the crank-pin and cross-head pin respectively, are:

$$F_1 = \frac{Wv_1^2}{3gr} \qquad F_2 = \frac{Wv_1^2}{6gr}$$

According to the first rule for the centrifugal force, F is here a third of the centrifugal force due to W. Hence, according to what has been said of the longitudinal force due to the pitman, the maximum lateral force on the crank-pin is a third of the maximum longitudinal force. Also the maximum lateral force on the cross-head pin is a sixth of what the centrifugal force of the whole pitman would be if hung centrally on the crank-pin.

2°. To make the lateral force on cross-head pin $= o$, make $F_2 = o$. Then suppose $w_1 = o$ and $b = o$, and we find:

$$a = \frac{l}{2} \text{ and } F^1 = \tfrac{3}{4} \frac{Wv_1^2}{gr};$$

that is, if the pitman be a uniform bar, with the crank-pin at a third its length from the end, the lateral disturbing force at the

cross-head pin will be nil, while at the crank-pin it will be three-fourths the longitudinal force at D.

3°. But retain w_1, that is, let the pitman overhang a short distance a, and carry a weight w_1 at the end, also let $b = o$, and $x = a$. Then we find for

$$F_2 = o, \quad \ldots\ldots\ldots \quad \frac{l}{a} = 2 + \frac{6w_1}{W}$$

In this expression the centre of gravity of the weight w_1 is supposed to be at the end of the overhang a.

Various relations are possible here. To illustrate, suppose

$w_1 = o$, then $l = 2a$ as before.

Again if $l = 5$, $W = 100$, $w_1 = 10$; $a = 1.93$,

Or " " 25, $a = 1.4$,

and the lateral force on the crank-pin can readily be found for each case.

4°. Next suppose a and $w_1 = o$, leaving b to overhang and make $F_2 = o$. This gives an equation from which we find the overhang b, the same as a in "Case 2°," viz.:

$$l = 2b \quad \ldots\ldots \text{ and } \ldots\ldots \quad F_1 = \tfrac{1}{4}\frac{Wv_1^2}{gr}$$

that is, the lateral jerks upon the cross-head pin are zero for a prismatic pitman when it extends beyond the cross-head pin a third of its length ($l + b$).

If it were desirable to find how great an overhang, a or w, one or both, is necessary to make the outward force on the crank-pin constant for the revolution, or to make the longitudinal force at D equal to the lateral at E, Fig. 77, we have the force at D, when $x = a$,

$$(W + w_1)\frac{v_1^2}{gr} = F_1. \quad \text{Whence } \frac{a}{l} = \sqrt{\frac{1 + \frac{w_1}{W}}{\frac{1}{8} + \frac{w_1}{W}}} - 1$$

$$\text{If } w_1 = 0 \quad \frac{a}{l} = .732 \quad F_2 = \frac{Wv_1^2}{gr}(-.134)$$

$$w_1 = W \quad \frac{a}{l} = .225 \quad F_2 = \frac{Wv_1^2}{gr}(-.163)$$

$$w_1 = \tfrac{2}{3}W \quad \frac{a}{l} = .291 \quad F_2 = \frac{Wv_1^2}{gr}(-.161)$$

From all these it appears that whenever the radial thrust upon the crank-pin is made constant, there will necessarily be a lateral jerking component upon the cross-head pin, and which will act

the opposite way, for each jerk, to that due to the ordinary form of connecting-rod considered in "Case 1°." If we put $\frac{a}{l}$ for "Case 3°" and "Case 4°" equal each other, we find $w^1 = -.25 W$, which cannot be practically realized. Hence it is impossible to make $F_2 = o$ and the crank-pin thrust constant at the same time.

Many of these results may be practically demonstrated by bending a common pin into a crank shape to whirl in the fingers, and by sticking it through a narrow strip cut from the edge of a card, with the strip brought up near the pin's head for a crank-pin, and the other end of the pin whirled between the fingers. The strip may be left free to the action of the crank. If the crank-pin is at one end of the card-strip, then a point at two-thirds the way toward the other end will nearly describe a straight line. Or if the pin be at a third the length, the farther end will nearly describe a straight line, etc.

COUNTER-BALANCING OF PITMAN.

Whenever the lateral and longitudinal forces upon the crank-pin are equal to each other in their maximum values, that is, the former at E, and the latter at D, as in Fig. 77, a counter-weight K may be so chosen as to perfectly counter-balance the forces acting on the crank-pin. But it is impossible to do this, and at the same time entirely counteract the lateral force on the cross-head pin. It is to be observed that when w_1 is not zero, the longitudinal force upon the crank-pin is due to $W + w_1$, instead of W.

To secure the nearest approach to perfect counter-balance of the pitman, choose a counter-weight, $K = \frac{1}{2}\,(W + w_1 + F_1)$ for the case of the radius $AK = AB$, in Fig. 77.

PITMAN AND RECIPROCATING PARTS COMBINED.

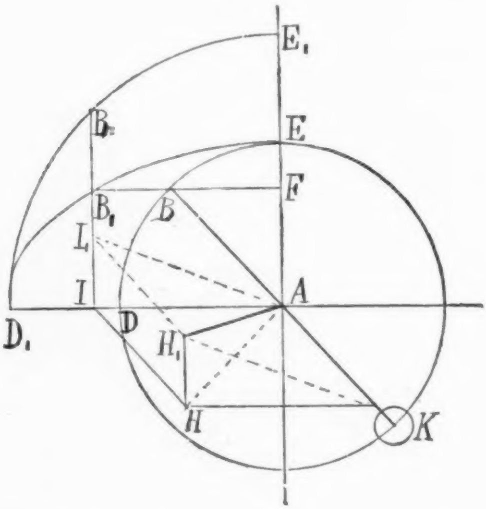
In practice there would usually be a pitman attached to reciprocating parts of some weight, as the piston, etc., in engines; or the saw-frame in saw-mills, etc. To realize the best counter-balancing effect, on crank-shaft, the longitudinal and lateral forces, or thrusts against the crank-shaft, for a revolution, should be equal to each other. This condition corresponds with that represented by the circle in Figs. 77 and 78.

If the curves in the present case for the resultant force at the main shaft are ellipses similarly as in Fig. 77, then the best condi-

tion gives a circle, and the counter-weight, $K = \frac{1}{2}(W + w_1 + C + F_1)$ where W is the weight of the prismatic part of the pitman; w_1 that of the weight overhanging at a distance a, as previously considered; C the weight of the piston and rod, cross-head, etc., and F_1 the lateral force upon the crank-pin, due to swing of pitman, etc., as previously determined.

But a little consideration will suffice to show that the curves are ellipses. In Fig. 80, draw the crank circle DBE. Then take the longitudinal force due to the piston and cross-head as BF,

FIG. 80.



as previously. We may lay off BB_1 to represent the longitudinal force due to the pitman. But this force varies according to the same law as BF; hence,

$$BB_1 : BF :: DD_1 : DA,$$

$E_1B_1D_1$ is an ellipse. This may be reduced to a circle, for convenience, by making $AD_1 = AD$.

But the force B_1B_2 varies according to the law of sines, so that the curve $D_1B_2E_1$ is an ellipse also; hence

$$B_1B_2 : B_1I :: EE_1 : EA.$$

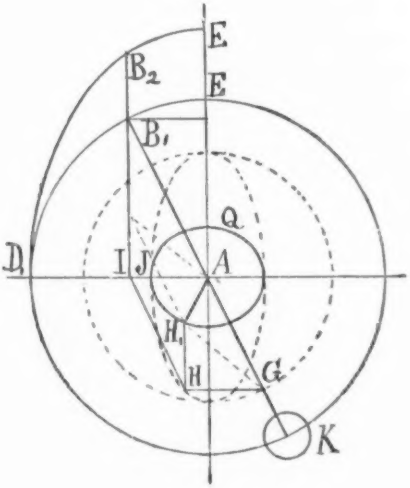
Now by drawing B_1I, and completing the parallelograms, AH is found to be the resultant corresponding to AH, in Fig. 77. But inasmuch as AD_1 may be taken the radius of the crank circle, draw

Fig. 12 and find the point H as in Fig. 77. Then take $HH_1 = B_1B_2H_2$ and H_1 is a point in an ellipse which gives any resultant for any point in the revolution. The point H is raised to H_1 by the fact that the lateral force of the pitman is $B_1B_2 = HH_2$, this force throughout the revolution serving to shorten up the ellipse, from the dotted one through H to the full line one through H_1, in Fig. 81.

The effect of this is seen to be to reduce the resultants AH; that is, the heavy pitman, together with its proper counter-balance K, has an effect to reduce the thrusts upon the crank-pin, and hence to reduce the shake, or give a nearer approach to a perfect counter-balance.

Hence it is preferable in high-speed engines to make the piston

FIG. 81.



and cross-head as light as possible, and put the weight into the pitman or connecting-rod; because the effect of the mass thus disposed, as regards cushion, absorption of power at beginning of stroke, etc., is the same as though it were placed in the piston or cross-head, and the heavy pitman possesses the second, and important advantage of reducing the shake.

The figure shows that to make the full line ellipse a circle, in Fig. 81, we must have

$$AJ = AQ$$

But

$$AJ = AD_1 - AG$$

i. e., equals centrifugal force of piston cross-head, pitman, etc., minus centrifugal force of K, or

$$= (W + w_1 + C)\frac{v_1^2}{gr} - K\frac{v_1^2}{gr}$$

. . . . and $AQ = AG - EE_1$

equals centrifugal force of K minus lateral force of pitman on crank-pin, or

$$= K\frac{v_1^2}{gr} - F_1$$

the radius to the centre of gravity of K being taken equal the crank r.

Hence the necessary counter-weight K is found from

$$2\,K = (W + w_1 + C) + F_1\frac{gr}{v_1^2} = (W + w_1 + C) + \left(\frac{W}{3} + w_1\right)\left(1 + \frac{a}{l}\right)^2$$

the value of F being introduced from the equation (1) above, making $b = o$, and $x = a$.

The jerking force at the same time is the radius of the circle, AQ, or AJ, or AH_1, which is now constant in value and revolves backwards. It is

$$\text{Jerking force} = AJ = (W + w_1, + C - K)\frac{v_1^2}{gr}$$

To illustrate, by example, take

$W = 50,\ w_1 = 0,\ C = 500,\ a = 0.$	Then $K = 283,\ AJ = 267.$
$W = 450,\ w_1 = 0,\ C = 100,\ a = 0.$	Then $K = 350,\ AJ = 200.$
$W = 100,\ w_1 = 350,\ C = 100,\ a = 0.$	Then $K = 467,\ AJ = 83.$
$W = 100,\ w_1 = 350,\ C = 100,\ \frac{a}{l} = .198.$	Then $K = 550,\ AJ = 00.$
$W = 200,\ w_1 = 102,\ C = 148,\ \frac{a}{l} = .198.$	Then $K = 396,\ AJ = 154.$

In each of the examples, the lateral thrust on the cross-head pin will be:

$$F_2 = \frac{v_1^2}{gr}\left\{\frac{W}{2}\left(1 + \frac{a}{l}\right) - \frac{W}{3}\left(1 + \frac{a}{l}\right)^2 - w_1\frac{a}{l}\left(1 + \frac{a}{l}\right)\right\}.$$

$F_2 = 8.3, 75, 16.7, 71$ and 00 respectively, in which $\frac{v_1^2}{2g} = 1$, and $x = a$.

In these examples it appears that the jerks upon the crank-shaft may be made zero, or that the lateral jerks upon the cross-head pin may be made zero, separately; but they do not seem to ever be zero simultaneously.

To test this point as a possibility we must have:

$$F_2 = 0 \text{ and } AJ = 0$$

Placing the values of $\frac{a}{l}$ found from each, equal each other we obtain:

$$\frac{w_1}{W} = -\frac{\frac{1}{4} + \frac{C}{W}}{1 + \frac{3\,C}{W}}$$

which is essentially negative, and not to be realized in practice. Retaining the overhang b makes the matter still worse.

Hence, it is impossible to so proportion the parts of an engine, formed as above specified, as to give an absolutely perfect balance throughout. But the jerks can be thrown wholly upon either the crank or cross-head, according to the wish or choice of the designer.

But, probably the best condition for quiet running of an engine is to so proportion the weights of the parts, W, w_1, C and K, as to make the longitudinal jerks zero, and at the same time make $A\,Q$ and F_1 the equal and opposite to each other.

The first condition makes:

$$W + w_1 + C = K$$

the radius of K being taken equal r.

The second condition gives:

$$K\frac{v_1^2}{gr} - F_1 + F_2 = o.$$

The effect of this adjustment is to give to the engine a jerking "couple," tending to make it rock or vibrate about its centre of gravity, and in the same time as the period of rotation of the engine.

A TRIANGULAR PITMAN.

Let us suppose the pitman so modified in form as to be a triangle, with its vertex at C, Fig. 79, and a short base c, at the extremity, $l+a$, the crank-pin being inserted at the middle of its width. Such a pitman is shown in Fig. 82. Then, with the notation above, we have:

$$F_1 = \frac{v_1^2 \sin\theta}{gr}\left\{\frac{W}{2}\left(1+\frac{a}{l}\right)^2 \pm w_1\left(1+\frac{x}{l}\right)^2\right\} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

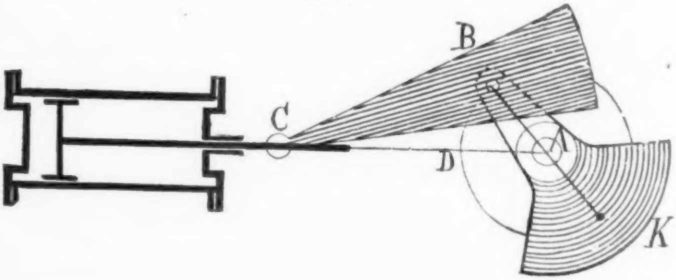
$$F_2 = \frac{v_1^2 \sin\theta}{gr}\left\{\frac{W}{2}\left(\frac{1}{3}-\frac{a}{l}\right)\left(1+\frac{a}{l}\right) - w_1\frac{x}{l}\left(1+\frac{x}{l}\right)\right\} \quad . \quad . \quad . \quad . \quad (4)$$

If

$$w_1 - o \text{ and } F_2 = o, \text{ then } a = \frac{l}{3} \text{ and } F_1 = \frac{8}{8}\frac{Wv_1^2}{gr}$$

which differs only one-ninth from the centrifugal force or longitudinal force due to the pitman. In the prismatic pitman the corresponding value of F_1 differed by a fourth instead of a ninth.

FIG. 82.



COMBINED TRIANGULAR PITMAN AND RECIPROCATING PARTS.

1°. Suppose $w_1 = o$ and for the shaking tendencies to all be zero, we must have, see Fig. 82,

$$F_2 = o \text{ and } AQ = K\frac{v_1^2}{gr} - F_1 = o = (W + C) - \frac{W}{2}(1 + \frac{a}{l})^2$$

From the first we get $a = \frac{l}{3}$, and from the second, by introducing this value of a, $C + \frac{W}{9} = o$, from which it appears that C or W must be negative, a condition impossible to realize in practice.

Hence the triangular pitman does not enable us to wholly destroy the jerking effects.

2°. Next, we observe that the best that we can do is to make these actions upon the cross-head and crank-shaft equal and opposite, while the longitudinal jerks are made zero.

This gives, for

$$w_1 = o, \ W + C = K,$$

and

$$K\frac{v_1^2}{gr} F_1 + F_2 = o$$

or,

$$o = W + C - \frac{W}{2}\left(1 + \frac{a}{l}\right)^2 + \frac{W}{2}\left(\frac{1}{3} - \frac{a}{l}\right)\left(1 + \frac{a}{l}\right)$$

from which

$$\frac{a}{l} = -\frac{2}{3} + \sqrt{\frac{10}{9} + \frac{C}{W}}$$

or,

$$\frac{C}{W} = \frac{a}{l}\left(\frac{a}{l} + \frac{4}{3}\right) - \frac{2}{3}$$

As this case is an important one, we bring the figures nearer to practice in the following results obtained by calculation: $\frac{v_1^2}{2g}$ is taken equal unity.

If

$\frac{C}{W} = o$,	then $\frac{a}{l} = .387$,	$AQ = W.037 = F_2,$	$K = W$
" $\frac{2}{9}$	" .488	$W.116 =$ "	" $\frac{11}{9}\ W$
" $\frac{4}{9}$	" .580	$W.195 =$ "	" $\frac{13}{9}\ W$
" $\frac{2}{3}$	" .666	$W.277 =$ "	" $\frac{16}{9}\ W$
" 1	" .786	$W.404 =$ "	" $2\ W$

These figures indicate that for a given diameter and stroke of cylinder of an engine, and for a given weight of a heavy pitman, the lateral jerking components, AQ at A in Fig. 81, or the equal and opposite ones, F_2 at C, in Fig. 79, increase with the weight, C, of the cross-head, piston, and rod; also, that the counter-balance weight, K, increases with C, from which it appears that the piston and cross-head should be made as light as possible, the weight being put into the pitman.

If we apply these results to engines in which high speeds are sought to be attained by the help of science as well as workmanship, as notable in the Porter-Allen engine, we should assume a certain weight of pitman, cross-head, piston, and rod for a given size of cylinder, speed, and steam pressure. That is, $W + C$ must be a constant, instead of W, as above. Making this change, the figures will be:

$$K = \text{a constant} = W + C.$$

Hence, for the same fractions for $\frac{C}{W}$ and $\frac{a}{l}$ we get

$$
\begin{array}{ll}
AQ = K.037 & W = K \\
AQ = K.094 & W = \frac{9}{11}K \\
AQ = K.134 & W = \frac{9}{13}K \\
AQ = K.156 & W = \frac{9}{16}K \\
AQ = K.202 & W = \frac{1}{2}K
\end{array}
$$

These show that the engine will be subject to much less jerking action when the pitman, or connecting-rod, is made very heavy, and the other reciprocating parts as light as possible. Hence, in practice, the cross-head, piston, and rod should be designed first, and reduced to the minimum in weight. The pitman is to receive the whole additional mass necessarily added to gain the desired weight of "heavy reciprocating parts."

As a more definite example, take an engine of 14″ × 24″ cylin-

der, 150 rev. per min., steam at 90 lbs. apparent, and $W + C = 720 + 100 = 820$ lbs. $= K =$ counter-balance weight. Then $\frac{a}{l} = .444$, $AQ = -F_2 = 168$ lbs.

Or again, interchange the weights, so that $W + C = 100 + 720$, to see the effect of the more usual proportions. Then $\frac{a}{l} = 2.22$, $AQ = -F_2 = 1212$ lbs.

In this example the engine will be nearly eight times as unsteady in position as in that of heavy pitman; also the pitman is made to overhang an inconvenient distance in the last example of over twice the pitman proper.

If we cut off the overhang of pitman in the last example, it will nearly agree with proportions adopted in present practice. Then, if the circle of Fig. 77 be adopted, K equalling about $\frac{1}{2}(W + C)$ or about 400 pounds; and the jerking action at crank-bearing, in all directions, will be about 1600 pounds, nearly tenfold greater unsteadiness than in the example of heavy triangular pitman. The radius to the centre of gravity of K is supposed to equal the crank radius.

Again, if the engine has no counter-balance K, the longitudinal jerking force will reach the comparatively enormous figure of 3280 pounds.

The example of heavy pitman seems so important that Fig. 82 is given to better illustrate it. The pitman is to be very wide at the crank end, and to overhang the crank-pin a distance equal $.444l$, and the counter-weight K is to equal 820 pounds, the pitman equal 720 pounds, and the cross-head piston and rod, 100 pounds, as mentioned in the example. In this engine the jerking effect in the direction of $A\ C$ is zero. The lateral jerks at A are 168 pounds up when the crank-pin is up, and down when the crank-pin is down. At the same time the jerks at C are 168 pounds up when that at A is down, and down when that at A is up. Under these conditions if the engine were suspended free it would have a slight tilting motion about the centre of gravity of the whole engine, the angle of this tilt being about a third of a degree of arc. A rocking effect of this sort is evidently less effective in disturbing the engine than a displacing force, as one tends to rock the engine and the other to move it bodily.

But Fig. 82 is awkward and uncouth in the extreme, according to modern notions of what constitutes good engine design; too

much so to be adopted with a relish, notwithstanding its great advantages.

It has been stated that the steam pressure upon the piston, and its reaction upon the cylinder head, contributes nothing toward vibration of engine in any way, because equal and opposite, and in a direct central line. But the pressure of the cross-head upon the guides being at its maximum at mid-stroke, and in the same direction for both forward and back-stroke, will give cause for a tilting or rocking motion in the above design, and with a periodic time half as long as that due to the dynamic force concerned in the heavy pitman of Fig. 82. The extent of this will be about twice that above, or about two-thirds of a degree of arc. But this is common to all forms of engine. But as regards the dynamic action of reciprocating parts towards shaking the engine and destroying its foundation, the advantage of Fig. 82, as compared with the ordinary construction, is better than as 168 to 3280, or better than 1 to 20, in favor of Fig. 82.

OUTSIDE CONDITIONS AFFECTING THE COUNTER-BALANCE.

I come now to consider very important conditions affecting the counter-balance weight, and which exist outside of the machine itself. To illustrate: suppose a vertical engine is to stand on a brick pier built up through a basement story of a factory. This pier will probably be made small as allowable to economize room in the basement.

Now this engine should be perfectly free from jerks in the horizontal, as these would shake the pier and destroy it But vertical jerks of a very considerable intensity will act in a direct line with the pier, and be powerless to disturb it unless of intensity sufficient to lift the whole engine.

From these considerations it appears that the latter jerking effects, F_1 and F_2, notably F_1, should be zero at the expense of the longitudinal. That is, the ellipse in Fig. 77, should be reduced to a straight line running lengthwise the engine, or vertical.

Again, suppose the engine stands on a floor at some distance from wall or pier supports. The floor will easily spring vertically and vibrate; but horizontally, it is stoutly stayed by the walls all around, so that the whole building must vibrate to and fro, if the floor shakes any, as it evidently would not appreciably, in its own plane. Under these conditions, the same vertical engine should be counter-balanced in an entirely different manner, the longitu-

dinal shake being made zero at the expense of the lateral. That is, the ellipse in Fig. 77 should be reduced to a line running crosswise the engine's centre line, so that the jerks will all come in the plane of the floor. The problems already worked out above, indicate how not only to reduce the disturbing forces to a line in either the horizontal or vertical direction, but how also to make the length of that line the shortest possible.

What has been said of engines is also true of jigsaws, mortising machines, and the like.

In discussing this subject I have not attempted to exhaust it. It is simply the balancing of pieces that are acting in one plane, and not going out of that plane—that case involving centrifugal couples. There is enough in that for perhaps quite a treatise. I confine myself to only a few instances of balancing of parts acting in one plane, where there are no couples at all to contend with.

DISCUSSION.

MR. PORTER: In venturing to open the discussion upon this paper—a discussion which I hope will be full, and in which many persons present will be able to contribute very valuable suggestions—I would like to say that I have been surprised in reading mechanical papers, sometimes, to see how very misty and wild the ideas of persons are upon a subject which seems to me to be so exceedingly simple. I think the simplicity of the subject ought to be made manifest, and popular ideas ought to be clarified, and the mists that surround it in the popular view ought, if possible, to be brushed away. The question is, how can this be done? I would like to say a few words, some of which may seem to those who have given special attention to the subject, to be quite elementary, and the first question that presents itself is, why do we balance a horizontal engine? I refer not to locomotives, but to stationary engines, and the answer to that question I apprehend to be, that it is simply to keep it from shaking. Counter-balancing contributes nothing whatever to its effectiveness as an engine. The distribution or application of force is not in any manner affected by the counter-balancing, except that, in a slight degree, a revolving counter-balance does add somewhat to the effect of the fly-wheel; but the object of the counter-balance is simply to keep the engine still. Now what is it that tends to shake an engine—to put it in vibration? The steam is admitted between the piston and the cylinder-head, and its pressure is equal in all directions;

action and reaction are equal. So far as work is done, there is no tendency to shake the engine. If we suppose the reciprocating parts to be without weight then, no force being employed in putting them in motion, the entire pressure of the steam is exerted, through the crank-pin, on the main bearing, and we have pressure on the main bearing in one direction and reaction of the steam on the cylinder-head in an opposite direction, in equilibrium exactly, whether the work be little or great. But in so far as the pressure of the steam does not reach the main bearing and exert pressure there—so far as it is absorbed in putting the reciprocating parts in motion—we have action against the piston and reaction against the cylinder-head, representing absolutely the case of a projectile being fired from a gun; the projectile moving and the gun recoiling in opposite directions, the force of the gunpowder being equal in the two directions, precisely as the force of the steam is equal in each direction. Now, how shall this recoil of the engine be neutralized? The obvious and perfect way of doing this would be by having two engines opposite each other, so that the action of the steam against the cylinder-heads would be equal and opposite. If the reciprocating parts were of equal weight, that would be an absolute and perfect balance. Mr. Sickles once made an engine in that manner, long before the war. He had two horizontal engines on a government vessel, the crank being of a threefold form, substituting a perfectly balanced engine. The Wells balance-engine is a perfectly balanced engine on the same principle exactly. But ordinarily, we are obliged to balance the reciprocating parts by means of a revolving counter-weight. Now practically, in using the revolving counterweight, we divide the radial action of the counterweight into its horizontal and vertical components, the vertical component being ignored altogether. We are obliged to do so; it is not possible to consider it. We consider the horizontal component only, and it is found theoretically and practically, that if a counterweight be placed opposite to the crank-pin—its centre of gravity no matter where—but of such a weight, and the weight so distributed, that at a point opposite to the crank and equidistant from the centre, its effective weight shall be equal to the entire weight of the reciprocating mass—piston and rod, cross-head, and connecting-rod—then we have an engine perfectly balanced in a horizontal direction, and it can be run at any speed whatever, and will maintain entire stability in a horizontal direction.

But in that case we have an equal vertical action of the counterweight which we are disregarding. That vertical action is partly counterbalanced by the opposite vertical action of the connecting-rod, which Professor Robinson has dwelt upon, and the method of making a connecting-rod to meet that vertical action suggested by the Professor is, no doubt, theoretically, the proper one to adopt, but practically it seems to be rather objectionable. I once went so far as to make a drawing, but I never went any farther than that. It did not seem to me that anybody would have an engine with such an affair vibrating on the crank, however useful and valuable it might be. I never ventured upon the experiment, at any rate. But undoubtedly, theoretically, it is the very thing to do to neutralize, in as great a degree as possible, the vertical action of the counterweight. But really, we do not need this heavy mass on the connecting-rod; we do not need to neutralize the vertical action of the counterweight in a stationary engine, because the action of the counterweight is resisted in one direction by the whole mass of the earth, and as for the opposite direction, the engine can be held to the earth; and as the counterweight cannot cause the engine to jump vertically, it is found not to be a very serious matter to do that. So that practically, in the case of engines fixed on good foundations, this difficulty does not seem to exist. We all know perfectly well that there is a counterweight revolving here which is exerting a disturbing action, but that action the earth completely resists.

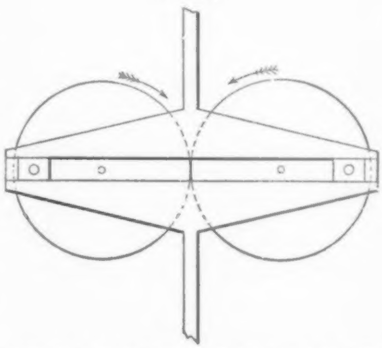
But the question arises, to what extent shall we balance the reciprocating parts of an engine? Now it must be borne in mind that the only office of the counterweight is to keep the engine still, and when we have got enough to keep the engine perfectly still, under the conditions existing, we have got counterweight enough. And then we do not have that excessive vertical action that we should have if we had counterweight enough to balance the engine, if it were free to move. To ascertain what is enough is a matter of experiment, and I am sorry to say that such experiments are not always to be relied upon. No two results can be got alike, under the different conditions as they will exist in practice. I have sent out engines that were sufficiently balanced, and others precisely like them which stood in different places on different kind of ground would be very insufficiently balanced. I remember that when I began making engines in New York, I put up an engine near the foot of 106th Street, and to my astonishment it shook very badly.

I never dreamed of the cause, but I increased the counterweight very much and managed to make it still; and then I supposed from that one observation that I should have to employ counterweights of a very inconvenient character indeed. I found out long afterwards that the trouble with this engine was that it stood on made ground, and a similar engine standing on good, firm earth was absolutely still. So that if one wants to meet the worst conditions he must approximate rather more closely than is convenient sometimes to a complete counterbalancing of the engine.

The counterbalance, as affected by the vibration of the connecting-rod, in another sense, is a very interesting thing. Of course the action of the counterbalance is equal in both directions; but the velocity put into the reciprocating mass is not equal in both directions by any means. It is far greater at the end of the cylinder farthest from the crank, and less at the opposite end. Consequently, if we have an engine exactly balanced, we shall have a constant excess of pressure in the backward direction from two alternate causes, one the excess of accelerating force, and also, on the other half of the stroke, the excess of counterweight. So the engine when it starts to revolve, if free to move, begins to run backwards with a smooth and steady motion, and the rate of motion will increase as the velocity of revolution increases. I have tried that experiment and it is very interesting.

The fact that a revolving weight will counterbalance the reciprocating parts perfectly, at every point of the stroke, in a horizontal direction, is a very interesting fact, and it arises from this,— the horizontal component of the centrifugal force of the revolving weight varies of course as the cosine of the angle it forms with the line of centres, and this corresponds precisely with the distribution of the acceleration of force on the piston, so that as the horizontal component varies, the accelerating force on the piston varies, and the two are in equilibrium for all speeds and at all points of the stroke.

FIG. 88.

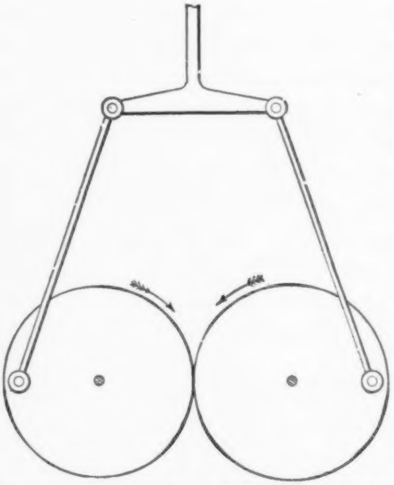


Arrangement of parts for perfect counter-balancing.

Mr. Partridge: It is a wonder that the makers of high-speed saw-mills have so long contented themselves with imperfectly balanced mills. Theory says that the pitman cannot be satisfactorily balanced, but I cannot see why this should in any way be allowed to interfere with perfect counterbalancing, for it does not appear that the pitman is an absolute necessity. If a pair of disk cranks are placed in the same plane and worked in the same slotted cross-head, and have their edges toothed so as to revolve in the same directions, then it will be possible to introduce counter-balance weights which shall be exactly equal in effect to the saw-frames, saws, and other reciprocating parts. Fig. 83 illustrates the point.

In this case there is no strain upon the guides of the slotted cross-head, as all the forces are in couples and balanced. It would make no difference whether the cranks were driven by the cross-head or were themselves the drivers. Even with the pitman there seems to be no reason why all the parts of a saw-frame should not be balanced, and even the pitman itself might be, as is shown by Fig. 84. Two disk cranks, as before, are made, by gear or otherwise, to revolve in the same direction. A pitman from each then connects them to a common cross-head.

Fig. 84.



Arrangement of parts for perfect counter-balancing with connecting rods.

In this case, as in the other, the disturbing forces are all made equal and opposite.

Professor Robinson: I would like to speak of one point that I thought Mr. Porter would bring up, but he seems to have over-looked it. If an engine is placed in a location where there is a tendency to vibration of the surroundings, at certain times if it be almost perfectly balanced and revolving in the same period as the time of vibration of the surroundings, there would be a certainty of serious movement in such a case. Whereas, if the speed of the engine be slightly different from the vibration of the surroundings, there would be scarcely any. Some cases of

vibration, which cannot otherwise be accounted for, may be explained on this score. If you drop a weight of eight or ten grains suspended from a thread on the blade of a hand-saw, one end of which is held in a vise, you will hardly notice any deflection, but keep dropping and lifting it repeatedly, keeping in time with the vibration, and presently you get the saw into quite a state of agitation. The story of the fiddler breaking down a bridge is an illustration of the same thing. I suppose the vibration is the same as that caused by the trot of a dog across a bridge, which I have known to cause considerable tremor, and doubtless to strain a bridge more than a cart would do.

THE PRESIDENT: Does not Mr. Woodbury know something of the breaking down of the Pemberton Mills? Was not that supposed to be caused by the rhythmic motion of the looms? There were defective columns there, but I presume—

MR. WOODBURY: There were defective columns. I have a sketch, but not with me, showing the construction of the columns of the mill. There were a number of causes tending to weaken that mill. I have no doubt from what I have heard of the matter from eye-witnesses of the fall of the mill, that the last straw which broke the camel's back was vibration.

THE PRESIDENT: My impression was that the mill was strong enough to bear all its static load without danger, but that the rhythmic vibration of the flooring under the looms carried it down finally.

MR. WOODBURY: If any of the gentlemen wish, I will have tracings, and copies of the statement of Mr. Thomas Bennet, which is the most complete one I ever saw, made and sent to them.*

THE PRESIDENT: It would be an interesting thing to bring out at the next meeting. This counterbalancing affects a very wide range of machinery. I would like to ask Mr. Porter if he ever built a vertical engine and counterbalanced pressure in it?

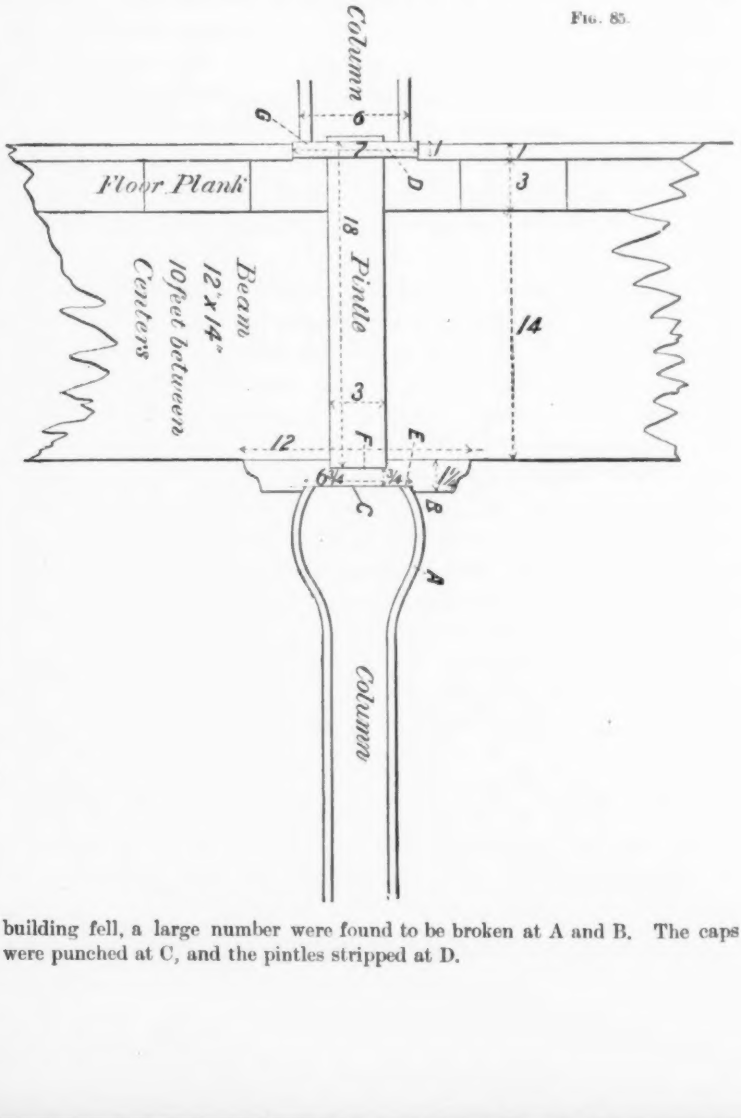
MR. PORTER: I built one a long time ago. About the counterbalancing of vertical engines I suppose the same rule must be observed as with horizontal engines, namely, to balance the horizontal

* SKETCH OF COLUMNS OF PEMBERTON MILL, WHICH FELL JANUARY 12TH, 1860.—Width of mill, 84′ 0″ outside. Two lines of columns, supporting beams, 10′ 0″ between centres, and 26′ 10½″, and 26′ 6⅝″ span. The lower columns supported four floors and the roof, brick piers being under the first floor. The height of the cornice, above underpinning, was 68′ 0″. The walls commenced with 2′ 6″ thickness at the water table, and terminated with 1′ 6″ at the roof. The caps were rough castings, and none of the bearings between caps, pintle, and columns were turned or finished in any manner. In casting the col-

forces and let the vertical ones take care of themselves. Although if there is a vertical engine whose weight and foundation will not prevent vertical vibration, then I suppose vertical vibration must be neutralized by other means; as in the case of the ordinary beam engine where the opposite movements do balance each other. But undoubtedly there is one horizontal action that needs to be balanced, which is the vibration of the connecting-rod. In addition to the balancing of the crank itself, the heavy end of the

umns the cores floated, and at A and B many of them were one-eighth of an inch thick, and in no place were they over three-fourths of an inch thick. After the

FIG. 85.



building fell, a large number were found to be broken at A and B. The caps were punched at C, and the pintles stripped at D.

connecting-rod needs to be balanced. And that counterweight, again, has a vertical action, which is not regarded, because it is supposed to be perfectly resisted by the earth.

This matter of the vertical action of the counterweight cannot be overlooked except in the case of engines which are securely fixed to their foundations. The most serious trouble locomotive builders experience is what to do with the vertical action of the counterweight, and I hoped to hear from some of our friends present, whose experience must be exhaustive in that matter, some of the results of their practice. I remember once seeing a steam fire-engine set on springs and nicely balanced horizontally, which went into a state of violent convulsion in a vertical direction, so that there was no possibility of running it.

Professor Thurston: The idea I had in my mind when I asked the question was whether it would under any circumstances be advisable to attempt to counterbalance pressures,—to render uniform the pressures on the crank-pin in a vertical engine after the manner that you render more uniform the pressures on the crank-pin in a horizontal engine. The card of pressures on the pin in the Porter engine is a comparatively uniform card, and it is obtained by making use of the weight of the reciprocating parts. Now in regard to a vertical engine, the question arising in my mind is, whether it is ever advisable to attempt to make those pressures uniform in such an engine. The horizontal engine seems peculiarly well adapted to that method, while the vertical engine seems peculiarly ill adapted to it.

Mr. Porter: There is this difference between vertical and horizontal engines, that in the latter we take no account of gravity, while in the former we have gravity assisting the downward and resisting the upward motion. So the problem is not by any means a simple one. I have never had occasion practically to apply the principles of balancing, or rather, the action of the reciprocating parts, to the vertical engine. I believe it can be done, but I fancy that it never will be perfectly done except by a corresponding weight acting in the reverse direction, as, for example, the air-pumps of marine engines which are connected with the cross-head through working-beams, giving to the piston of the air-pump a motion the reverse of that of the piston of the reciprocating parts of the engine. By adding a sufficient weight there to balance the reciprocating parts, I suppose they could be perfectly balanced, and the influence of gravity would be neutralized.

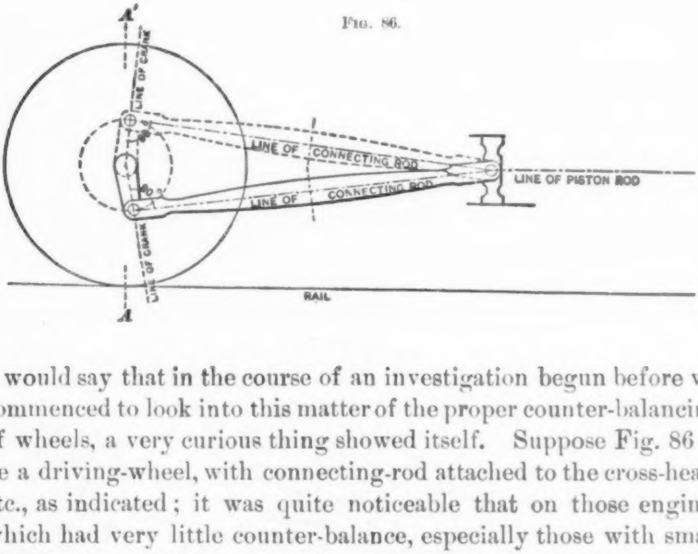
PROFESSOR THURSTON: I remember that Captain Ericsson in one of his ships built during the latter part of the war—the Madawaska—attempted to counter-balance by vibrating masses of metal. The result was not on the whole satisfactory, I think, although there was an advantage in the use of the counter-balance; but the idea was precisely what has been just described.

MR. VOGT: The formula that we make use of is that of D. K. Clark, as given in his book on Railway Machinery. We have found it necessary, however, to modify it slightly. The formula as given in his book is as follows: "Find the separate revolving weights in pounds, of crank-pin, coupling-rods, and connecting-rod for each wheel; also the reciprocating weight of the piston and appendages, and half connecting-rod. Divide the reciprocating weight equally between the coupled wheels, and add the aliquot part so allotted to the revolving weight on each wheel. The sums so obtained are the weights to be balanced at the separate wheels. Multiply by the length of crank in feet, and divide by the radial distance in inches of the centre of gravity of the space to be occupied by the counterweight. The result is the counter-balance in pounds to be placed exactly opposite the crank."

We apply this rule to both passenger and freight engines. In a large class of freight engines, called "consolidation" engines, we found it impossible to obtain sufficient counter-balance, partly on account of the small size of the wheels, and also on account of the great size of the rods, crank-pin, etc., and piston. But on passenger engines having wheels five feet, or five and a half feet diameter, we found no difficulty in giving the full amount. A curious thing developed itself, however, when engines were running at high speed, and which was most noticeable in going over bridges, namely, a vertical vibration of the whole bridge. Looking into the matter we discerned very clearly the cause of it. It is this: after you have counter-balanced the revolving weights, you add a certain amount for the reciprocating weights, which destroys the counter-balance in a vertical direction. Now here is the difficulty: you have to add something for the reciprocating weights or you will get a jerky motion in the train. This is observed particularly where diagrams are taken with a dynamometer registering the pull. It is very noticeable on the diagram. And if you will keep watch of the beats of the engine you will find them always coincide with these vibrations of the pencil, showing there is not enough weight to counter-balance the reciprocating parts.

In the last design of engines that have been made, we thought we would try to modify it a little, and have only taken two-thirds of the reciprocating weight and balanced that, and the result is quite good. It is impossible to make it perfect, because if you put in the full amount you get out the other way. You of course destroy the direct motion forward and backward, but you get another effect, which is, perhaps, more destructive to the road-bed. It is not felt in the train at all, but itall appears in the heavy strains on the bridges and in destructive pressure on the rails. The mode of putting in the counter-balance is the usual one,—making a cavity in the wheel and filling it with lead. In some cases we have put in cast-iron blocks. These are not very good. They are apt to get loose. I do not know that I have very much more to add.

FIG. 86.



I would say that in the course of an investigation begun before we commenced to look into this matter of the proper counter-balancing of wheels, a very curious thing showed itself. Suppose Fig. 86 to be a driving-wheel, with connecting-rod attached to the cross-head, etc., as indicated; it was quite noticeable that on those engines which had very little counter-balance, especially those with small wheels, the periphery was quite frequently worn, in the relative position to the crank, shown by the dotted line. On the other wheel, it was elsewhere, showing that it could not be due to the slip, because the flat spots did not coincide on the two wheels. The only thing I could attribute it to was the hammering action of the unbalanced weight of the crank-pin and rods.

THE PRESIDENT: How do you account for its not appearing in line with the crank radii?

MR. VOGT: I cannot account for it.

MR. WOODBURY: Is not that the point of maximum downward thrust?

MR. VOGT: That may have something to do with it. There is a thing observable in locomotives—viz., that when moving in a forward direction, every time the wheels revolve there is a strain brought on the road-beds, rails, and bridges—which is very seldom taken into account when the bridges are constructed. This is the vertical component of the thrust along the connecting-rod, and it has been thought that in some cases where bridges were broken, it was to some extent due to that. When an engine runs very fast,—perhaps five revolutions a second,—at every turn there is a downward shock, and those shocks coming at regular intervals may cause very serious trouble in a weak bridge.

PROFESSOR ROBINSON: Have the vertical jerks ever been found great enough to lift a wheel from the track?

MR. VOGT: I have never noticed that. It would not be very likely that there would be a lift, because the preponderance of the weight is downward, and it would not be very likely that the wheel would be lifted up from the track. A number of years ago a class of engines were much in favor in France that had enormous driving-wheels, the axle of which was located right behind the fire-box, and one road built its own engines and counter-balanced them to the full extent. In several cases they ran off, and it was directly traced to excessive counter-balancing of their reciprocating parts, causing the wheel to have less adhesion, and to have a tendency to leave the track, if other circumstances were favorable. They reduced the weight and had no more trouble, and that was what partly caused us to reduce the weight of the larger driving-wheels.

MR. OBERLIN SMITH: I would like to ask Mr. Vogt if he thinks there is any particular advantage in an engine, which he may have seen in the *American Machinist* some time ago, known as the "Shaw" locomotive. Is it considered by railroad engineers a novel and valuable invention?

MR. VOGT: It is almost the same as the Wells engine, of which Mr. Porter spoke. The idea is almost the same, but carried out in a slightly different manner. I think the construction is a very doubtful one. I don't think that it will ever prove to be a very strong construction. It is, no doubt, a very convenient way of counter-balancing everything, but it will never be an economical engine. You have two cylinders and a considerable condensing surface, and it is certainly better to have one cylinder. I am much

afraid it is not going to be a very great success. A number of years ago Mr. Haswell, of the Austrian State Railways, brought into use an engine somewhat similar. He had, however, two cylinders, one on top of the other, but I don't think it was worth anything. I think it was abandoned.

THE PRESIDENT: Do you know anything about the Carel locomotive, arranged so that the motion of the piston was forward, when the motion of the coupling-rods was backward?

MR. VOGT: Yes; something like it has been brought out by Brown, of Winterthur, for small street locomotives. His object was to raise the cylinders up so as to get them away from the dirt. He makes use, by the way, of a very interesting valve-motion. It is interesting through being rather a unique thing. It is a combination of the parallel-rod motion and of a system of connecting levers. He was enabled to use it only by having his cylinder very high up.

MR. SMITH: Can you tell me when that was gotten up?

MR. VOGT: Two or three years ago.

MR. SMITH: It reminds me, very forcibly, of a little device I invented in 1869. This was a small four-horse engine. I don't know that it was good for anything except to reduce the ordinary slide-valve engine to the utmost simplicity of construction. It was of the inverted vertical type, but any other style could have been built in a similar manner. There was a rock-shaft, parallel with main shaft, set so that the end of its short arm came directly underneath the slide-valve rod. Its *long* arm, nearly at right angles to the short arm, was attached by a short pitman to the main crank-pin, which was, of course, prolonged sufficiently for the purpose. The rock-shaft was set enough higher than main shaft to give the proper *lead*. Thus the crank itself was the eccentric. This engine ran very satisfactorily for several years.

MR. LYNE: Referring to the Shaw engine, I would like to mention one or two important features which I noticed in that engine last spring. One was that in crossing bridges there was no evidence of vertical vibration. So far as the balancing of that engine is concerned, it is the nearest perfection of anything that I ever saw. But since I rode upon it one of the double cranks has broken off. The double crank is attached to the driving-wheel by a solid connection with the pin. The double crank and the pin are one forging. I believe it has been proposed to make the connection between the double crank and crank-pin, and the wheel, by brasses or something that is flexible, which will prevent the vibration be-

ing taken up in the metal, causing it to become crystallized and to break off. Shortly after the engine was made it was raised on four jacks, at the Hinkley Locomotive Works, and they ran it at a speed of two hundred and seventy-five revolutions per minute, and there was no evidence of vibration, either vertical or horizontal, all the parts being exactly balanced with each other. But it is a problem among railroad men whether the advantages gained in that direction will compensate for the losses which are supposed to occur through the cylinder condensation and otherwise. The railroad men on the Providence & Boston Railroad informed me, however, that this engine was burning less coal and using less water than a 17 × 24 inch engine, which was drawing six cars and making the same time. The cylinders upon the Shaw engine were 10½ inches in diameter and the stroke was 22 inches.

Mr. Stirling: I am sure that I only express the feeling of all who are here when I say that it is most satisfactory to have some locomotive men in our society. I think that the practice of locomotive engineers will throw a very great deal of light on stationary practice. Take this method of balancing, for instance. Locomotive men have opportunities of experiment and observation that stationary men have not. We don't know so well whether our engines are balanced or not. The locomotive men necessarily are compelled to have their engines very perfectly balanced. As to the paper our President read recently, in regard to the governing of the engine by changing the compression, I have no doubt that some of our locomotive members can throw a great deal of light on the benefits of compression, because in no place is compression carried to such an extent as on the locomotive. I would like to ask some of our locomotive men as to the consumption of coal per horse-power per hour on the locomotive. What is the average, or a good rate of, combustion per horse-power per hour on the locomotive?

The President: Can Mr. Vogt tell us that?

Mr. Vogt: I cannot give any definite data, but only a few days ago I saw a statement, made by one of the principal locomotive engineers of England, that two pounds of coal per horse-power per hour was sufficient.

Mr. Stirling: I would like to have Mr. Vogt give his own impressions about that.

Mr. Vogt: I would not like to say anything at all on that subject, because, really, I do not know. I do not think there are any

data in the hands of Mr. Cloud, who has charge of that department, that can show exactly what it is. He promised us some time ago that he would figure out the amount of coal per horse-power per hour used on the new engines, but I don't think he has done so yet. I have not had time to do it myself. Mr. Cloud, I have no doubt, will be able to get at it, and perhaps he will have something to say on the subject.

THE PRESIDENT: Possibly he may be able to give us something on the subject at our next meeting. Mr. Lyne ought to be able to tell us something about it.

MR. LYNE: I made some experiments on a locomotive on the Delaware & Lackawanna Railroad; but I found, in the first place, that my instruments were not what they ought to be, and I had to perfect them. Then I found that the packing used in the cylinder was cast iron, and of such a construction that when the engine was working heavily a cloud of iron was flying about in the cylinder all the time. Under the circumstances I thought I could not get any results showing the average duty of locomotives in this country. When I arrived at one of the destinations I had a cylinder-head taken off and found one of the cast iron packing rings broken into fifteen pieces. I counted the pieces as they were taken out. The Railroad insisted on getting its money's worth out of the packing, however. It is certainly a bad packing to use. It scratches out the bottom of the cylinder. I noticed in one case the cylinder was worn out at the bottom five thirty-seconds of an inch more than at the top. In other words, the bore of the cylinder was five thirty-seconds of an inch lower than it ought to have been. In some cases, however, it works very well; for instance, where the iron composing the cylinder is quite hard and the rings soft. Under those circumstances it appears to produce good wearing surfaces. But the engine I was on was in such a bad condition that I had to give up what I wanted to do. I have a partially completed arrangement by which I expect some time during the fall to make some further experiments, with the hope of getting at that very thing. I do not know of any reliable data on the subject.

THE PRESIDENT: Cannot Mr. Stratton tell us how much locomotives burn per horse-power per hour?

MR. STRATTON: I think that is something which has not yet been figured up. We figure our expenditure by the train mile.

THE PRESIDENT: It seems that the thing has not been worked

very completely. The best thing that I have known to be done in non-condensing engines has been done in portable engines. At some of the English agricultural shows they have got down to 1.5 pounds per horse-power per hour with portable engines.

MR. WOODBURY: I think there is one place in this country where they are getting better results, and that is at the East Hampton Rubber Thread Company. They have a Wright condensing engine. I was told that they were getting a horse-power for an hourly consumption of 2.1 pounds of coal. Having expressed some doubt in regard to the matter, the engine was put into my charge and I had access to the coal accounts of each day's consumption, and the result of indicator cards, taken during the day—much of the time every fifteen minutes—gave 2.06 pounds of coal per horse-power per hour, which is the best result I have ever had any personal knowledge of, in an engine in actual operation driving machinery. I know very well what has been claimed for certain performances at fairs.

THE PRESIDENT: That, however, is quite a different case from a locomotive engine with simple valve-gear. I presume it will be found that a good locomotive will give about three pounds. There are many cases on record of condensing engines coming down to less than two pounds. I have received of late reports of test trials made by German and French engineers, which gave usually about 1.7 pounds. But in locomotive practice the custom of measuring the expenditure of fuel by the train mile has interfered with the working up of the horse-powers.

MR. WOODBURY: The Lynn engine consumed 1.65 and the Lawrence 1.63 pounds of coal, and 1.65 pounds of steam per hourly indicated horse-power.

MR. STIRLING: Professor Thurston has mentioned a matter which has something to do with the consumption of coal per horse-power per hour; that is, the rapidity of combustion, and a little experience that I have had lately leads me to think that the locomotive men really have the advantage over those who have to deal with stationary engines because of the rapid combustion. I think that on the whole the economical results will be better with a very rapid combustion under favorable circumstances than with a very slow combustion.

MR. PORTER: I think it is a pity, Mr. President, that we cannot have some very careful experiments by competent persons who have plenty of time, to give us a knowledge of the horse-powers

actually exerted, so that we may know the consumption of fuel, not per mile or per ton carried per mile, but the horse-power exerted. It is a very serious matter to obtain these data so reliably as to amount to anything. One must make a business of it and have nothing else to do, and have many different opportunities, on different roads and under varying conditions, of applying the indicator and taking measurements. Results obtained from one engine alone, however accurate they might be, would be of very little value. If this Society were in a position to appoint a committee of persons able and willing to undertake such a service, and have the work performed for its own use, for the consideration of the world and for the benefit it might be to the locomotive engineering profession, it would be an undertaking eminently worthy of such an association. I do not suppose that any Railroad company could be expected to undertake such an investigation; and if it were willing, it is not the performance of one road, it is the performance of a good many, and the average and the comparative results obtained on different roads, and with the different construction of engines, etc., that is needed. The results themselves are of no value, unless we know precisely the means by which they have been obtained, and are able to compare them. I submit that this Society should really take the matter up as one eminently worthy of its attention.

Mr. Oberlin Smith: I would observe, Mr. President, pertinent to what Mr. Porter has just said, that I stated at the last meeting that I would at this one make a motion for the appointment of a committee of our members to investigate the subject treated in a paper that I then read on "Experimental Mechanics," and say whether the Society ought to do anything in an experimental way; but I think, as the attendance is so small, that we would better postpone the matter until a future meeting.

The President: I have no doubt that such a committee, properly organized by the Society, would receive every courtesy from almost any Railroad, the performance of whose locomotives they would like to investigate. Properly, I should say, the Society ought to act as a committee of the whole, each individual acting in that direction, every man doing all he can.

XXXV.

Continued from page 217.

ON THE MOST ECONOMICAL POINT OF CUT-OFF IN STEAM ENGINES.

II.

BY ALFRED R. WOLFF, M.E., AND JAMES E. DENTON, M.E.

At the last meeting of the Society we read a communication on the above subject, wherein it was shown that the most economical ratio of expansion in a steam engine should be that ratio which would give the maximum efficiency of the engine for the least expenditure of *money*, rather than as had heretofore been generally assumed and advocated,—that ratio which would secure the maximum efficiency of the engine for the least expenditure of *steam*.

It became evident from the general character of the investigation, and from the typical examples cited, that the most economical ratios of expansion, when properly computed, would seem to result in much lower ratios than had been considered good practice.

In the discussion that followed, much of which introduced questions that had already been met and answered in the paper itself, the only objection advanced seemingly implying a doubt—not as to the correctness of the method, but as to the correctness of the illustrations and deductions—was that, at the present time, there existed no knowledge of the exact steam consumption in different types of engines at different ratios of expansion ; or what is identical, that there was no definite knowledge of the variable condensation of steam at different points of cut-off. In our communication, and in the discussion succeeding it, it was stated by us that an examination of this point had demonstrated that quite extreme variations of condensation at different ratios of expansion would not seriously affect the most economical point of cut-off. Errors in correct allowance for condensation would certainly influence the best ratio, but only so to a slight extent. No amount of condensation, as far as might be determined by experience or imaginable in practice, would cause the most economical ratios to even approximate to the higher ratios which had been considered and advocated as the most economical, both in practice and theory, when the question of efficiency of fluid alone had been considered. Since in the debate that followed the reading of our previous paper this

point was overlooked, and has since been challenged privately by those whose opinions might possibly retard the adoption of the method, we consider it well to show by a practical illustration to how slight an extent varying condensation of steam at different points of cut-off affects the best ratio to be employed, and to what extent the actual economy of the steam engine is affected by different percentages of condensation. We propose to do this, in this paper, by finding the actual cost per horse-power of an engine, at different points of cut-off, from full stroke to ten expansions, allowing first for no condensation, then for 10 per cent. greater steam consumption than called for by indicator card, and so up by intervals of 10 per cent. to 100 per cent. greater steam consumption than called for by indicator card.

Consider an engine with cylinder 26″ diameter by 48″ stroke; non-condensing; making 100 revolutions per minute. Steam pressure by gauge, 75.3 pounds; back pressure, including friction, 17.7 pounds, absolute. Take the price of such an engine set up in place, complete, at \$9000. The price of coal \$5 per ton of 2000 pounds. The wages of engineer at \$4 per day of ten hours. The cost of boilers at \$8500. The wages of one fireman and one coal-passer, jointly, at \$3.50 per day of ten hours. Other items of expense as below. Let steam follow full stroke; then allowing for clearance* ($2\frac{1}{2}$ per cent.) and neglecting condensation, we would have the expenditure per hour as follows:

38,430 pounds of steam, requiring 4270 pounds of coal at \$5 per ton of 2000 pounds,	\$10.675
Interest on cost of engine,	.062
Interest on cost of boilers,	.059
Repairs to engine,	.018
Repairs to boilers,	.023
Depreciation of engine, life assumed at twenty-five years,	.042
Depreciation of boilers, life assumed at twelve years,	.082
Oil and waste,	.062
Wages of engineer,	.400
Wages of one fireman and one coal-passer,	.350
Total expenses,	\$11.773

* In estimating the steam consumption, the allowance for clearance has been accidentally taken as proportional to the ratios of expansion, instead of allowing a constant quantity for clearance for all ratios of expansion. This trifling neglect produces no change of any significance in the results obtained, and does not affect the value of and the comparison shown in the table. The slight fluctuations noticeable in the figures of the column "horse-power per dollar of ex-

The work done will equal in horse-power: Mean effective pressure (absolute) per square inch $\times$ area of piston in square inches $\times$ twice the stroke in feet $\times$ revolutions of engine per minute divided by 33,000. Now, mean effective pressure in this case will be $90 - 17.7 = 72.3$ pounds per square inch. Therefore, horse-power will equal: $\frac{72.3 \times 3.1416 \times 13 \times 13 \times 2 \times 4 \times 100}{33,000} = 72.3 \times 12.871 = 930.573$.

We obtain 930 horse-power, therefore, at an expenditure of \$11.773, or $\frac{930.573}{11.773} = 79.043$ horse-power per dollar expended. Now, suppose we cut off at half stroke, then the expense may be taken the same, except the cost of coal, which as condensation is not considered for the present, will be one-half of \$10.675, or 5.337, so that the total expense per hour will be $5.337 + (11.773 - 10.675 = 1.098) = \$6.435$. The mean effective pressure for $\frac{1}{2}$ cut-off, if steam expands according to the adiabatic ($\frac{10}{9}$) law will be $.834 \times 90 - 17.7 = 57.36$ pounds, and hence the horse-power will be $57.36 \times 12.871 = 738.28$. So that we will derive $\frac{738.28}{6.435} = 114.729$ horse-power per dollar expended. Similarly we carry out the operation for different cut-offs, and for different percentages of condensation with the results noted in the table.

It will be seen that for convenience in calculation of table we have here considered the interest on cost, repairs, and depreciation of boiler, and the wages of the firemen, as all expended directly for the development of the work, or horse-power. In the method presented in our previous paper, the interest on cost, repairs, depreciation of boiler, and the wages of the firemen are charged as an element in the expense of furnishing the steam; though it will be found that the results obtained in this table vary insignificantly from the results obtained by the use of the graphical or analytical method displayed in our previous communication, which latter methods have the advantage of giving each item of expense its exact economical significance in relation to the economy of the engine.

From an inspection of this table it will be seen that the effect of a greater percentage of condensation in the cylinder, assuming the condensation constant at all points of cut-off, is to increase—though only slightly—the ratio securing the greatest economy of the engine, but that an increase of condensation with increase of expan-

pense," after passing the most economical point of cut-off, arise from the fact that the "ratios of initial to mean pressures" have been expressed in three decimals. If these had been expressed in five decimals, and calculations made on that basis, this trifling and insignificant fluctuation would not have appeared.

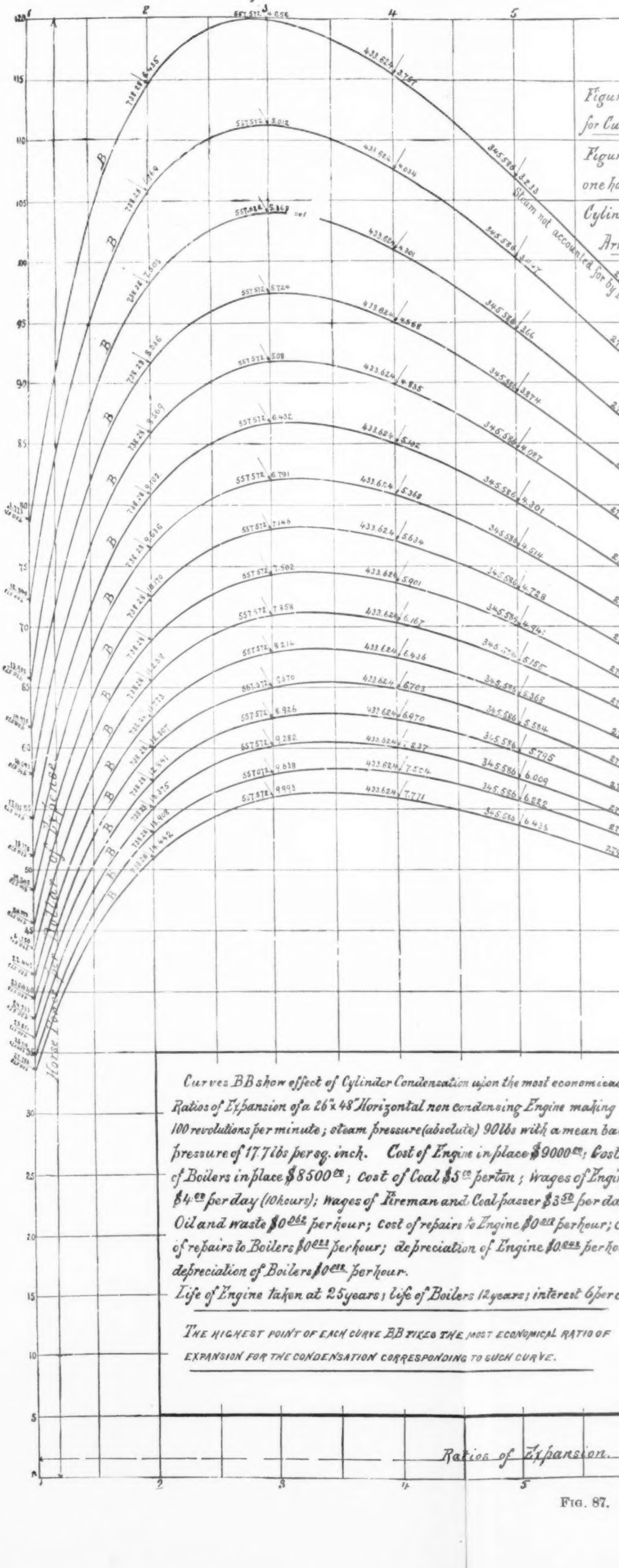

Horse Power per Dollar of Expense
Steam not accounted for by
Curves BB show effect of Cylinder Condensation upon the most economica
Ratios of Expansion of a 26" x 48" Horizontal non condensing Engine making
100 revolutions per minute; steam pressure (absolute) 90 lbs with a mean ba
pressure of 17.7 lbs per sq. inch. Cost of Engine in place $9000.00; Cost
of Boilers in place $8500.00; cost of Coal $5.00 per ton; wages of Engi
$4.00 per day (10 hours); wages of Fireman and Coal passer $3.50 per da
Oil and waste $0.062 per hour; cost of repairs to Engine $0.013 per hour; c
of repairs to Boilers $0.021 per hour; depreciation of Engine $0.045 per ho
depreciation of Boilers $0.012 per hour.
Life of Engine taken at 25 years; life of Boilers 12 years; interest 6 per c
THE HIGHEST POINT OF EACH CURVE BB FIXES THE MOST ECONOMICAL RATIO OF EXPANSION FOR THE CONDENSATION CORRESPONDING TO SUCH CURVE.
Ratios of Expansion.

Fig. 87.

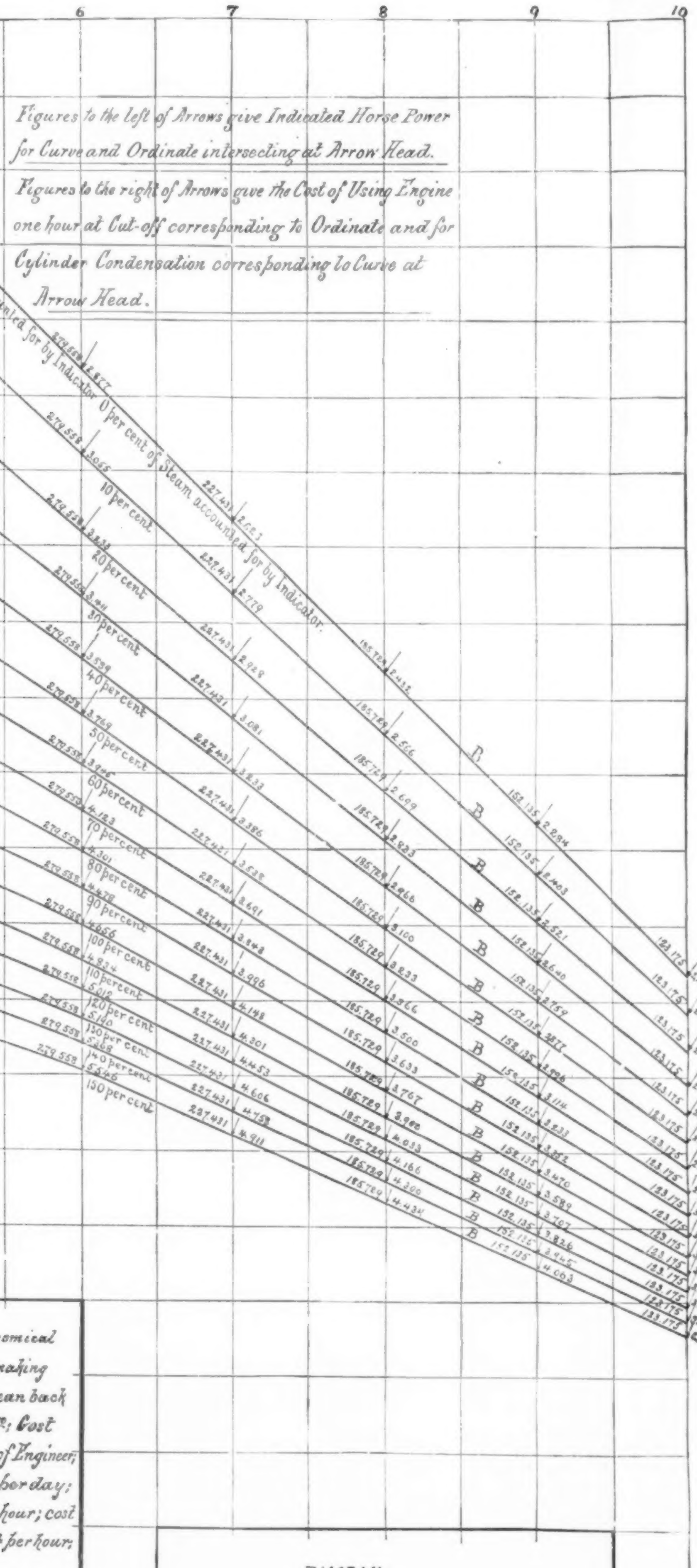

6
7
8
9
10
Figures to the left of Arrows give Indicated Horse Power for Curve and Ordinate intersecting at Arrow Head.
Figures to the right of Arrows give the Cost of Using Engine one hour at Cut-off corresponding to Ordinate and for Cylinder Condensation corresponding to Curve at Arrow Head.
0 per cent of Steam accounted for by Indicator.
10 per cent
20 per cent
30 per cent
40 per cent
50 per cent
60 per cent
70 per cent
80 per cent
90 per cent
100 per cent
110 per cent
120 per cent
130 per cent
140 per cent
150 per cent
B
DIAGRAM
To Accompany Paper on
The Most Economical Point of Cut-off in
Steam Engines
II
by Alfred R. Wolff and James E. Denton
Aug. 1881.

TABLE.

Ratio of expansion.	Ratio of initial to mean pressure.	Mean effective pressure.	Effective horse-power developed.	PERCENTAGE FIGURES INDICATE AMOUNTS BY WHICH STEAM CONSUMPTION EXCEEDS THAT SHOWN BY INDICATOR CARD. 0 per cent.		10 per cent.		20 per cent.		30 per cent.		40 per cent.	
				Total expense.	Horse-power per dollar of expense.	Total expense.	Horse-power per dollar of expense.	Total expense.	Horse-power per dollar of expense.	Total expense.	Horse-power per dollar of expense.	Total expense.	Horse-power per dollar of expense.
1.00	1.000	72.30	930.573	11.773	79.043	12.840	72.475	13.908	66.992	14.975	62.141	16.043	58.004
1.25	.976	70.14	902.772	9.638	93.668	10.492	86.044	11.346	79.567	12.200	73.997	13.054	69.156
1.50	.931	66.09	880.644	8.215	103.548	8.927	95.289	9.639	88.250	10.380	82.187	11.062	76.897
1.75	.881	61.50	792.725	7.198	110.131	7.808	101.827	8.418	94.169	9.028	87.807	9.638	82.249
2.00	.834	57.36	738.280	6.435	114.729	6.939	105.938	7.503	98.398	8.036	91.871	8.563	86.177
2.10	.815	55.65	716.271	6.181	115.883	6.689	107.073	7.097	99.523	7.705	92.961	8.214	87.201
2.20	.798	54.12	696.578	5.950	117.071	6.435	108.248	6.920	100.661	7.405	94.068	7.890	88.286
2.30	.781	52.59	676.885	5.739	117.945	6.203	109.122	6.667	101.521	7.131	94.921	7.595	89.122
2.40	.765	51.15	658.351	5.546	118.707	5.991	109.890	6.436	102.290	6.880	95.690	7.325	89.877
2.50	.743	49.62	638.659	5.368	118.975	5.795	110.200	6.222	102.645	6.649	96.053	7.076	90.257
2.60	.733	48.27	621.283	5.204	119.385	5.615	110.647	6.026	103.100	6.436	96.532	6.847	90.738
2.70	.719	47.01	605.066	5.052	119.768	5.447	111.082	5.842	103.572	6.238	96.996	6.633	91.220
2.80	.704	45.63	587.690	4.910	119.692	5.292	111.052	5.673	103.594	6.054	97.074	6.436	91.341
2.90	.631	44.49	572.631	4.779	**119.822**	5.147	111.225	5.515	103.831	5.883	97.336	6.251	91.606
3.00	.678	43.31	557.572	4.656	119.753	5.012	111.247	5.368	103.869	5.724	97.409	6.080	91.708
3.10	.666	42.24	543.671	4.542	119.699	4.886	**111.271**	5.230	**103.982**	5.575	**97.519**	5.919	**91.851**
3.20	.654	41.16	529.770	4.434	119.479	4.768	111.122	5.102	103.835	5.435	97.473	5.769	91.830
3.30	.642	40.08	515.870	4.336	119.056	4.657	110.773	4.981	103.567	5.304	97.250	5.628	91.661
3.40	.630	39.00	501.969	4.238	118.445	4.552	110.274	4.866	103.158	5.180	96.905	5.494	91.385
3.50	.620	38.10	490.385	4.148	118.222	4.453	110.125	4.758	103.065	5.063	96.856	5.368	91.355
3.60	.610	37.20	478.801	4.063	117.844	4.359	109.842	4.656	102.835	4.952	96.688	5.249	91.267
3.70	.600	36.30	467.217	3.983	117.177	4.271	109.393	4.560	102.459	4.848	96.373	5.137	90.950
3.80	.590	35.40	455.633	3.907	116.620	4.188	108.795	4.469	101.954	4.750	95.902	5.031	90.565
3.90	.580	34.50	444.049	3.835	115.791	4.109	108.067	4.383	101.311	4.656	95.371	4.930	90.670
4.00	.571	33.69	433.624	3.767	115.111	4.034	107.981	4.301	100.819	4.568	94.926	4.835	89.684
4.10	.562	32.88	424.198	3.702	114.289	3.962	106.814	4.222	100.236	4.482	94.421	4.742	89.244
4.20	.554	32.16	413.931	3.640	113.717	3.894	106.299	4.148	99.790	4.402	94.032	4.656	88.902
4.30	.545	31.44	404.641	3.581	113.003	3.829	105.683	4.077	99.255	4.325	93.561	4.574	88.470
4.40	.538	30.72	395.397	3.524	112.201	3.767	104.963	4.009	98.627	4.252	92.990	4.495	87.963
4.50	.530	30.00	386.130	3.470	111.276	3.707	104.162	3.945	97.878	4.182	92.331	4.419	87.379
4.60	.523	29.37	378.021	3.419	110.564	3.651	103.539	3.883	97.352	4.115	91.864	4.347	86.961
4.70	.516	28.74	369.912	3.369	109.798	3.596	102.867	3.824	96.734	4.053	91.268	4.278	86.468
4.80	.509	28.11	361.804	3.322	108.911	3.544	102.089	3.767	96.045	3.989	90.700	4.212	85.988
4.90	.502	27.48	353.695	3.277	107.932	3.494	101.229	3.712	95.284	3.930	89.998	4.148	85.244
5.00	.495	26.85	345.586	3.233	106.862	3.447	100.257	3.660	94.422	3.874	89.206	4.087	84.557
5.25	.479	25.41	327.052	3.131	104.456	3.335	98.066	3.538	92.439	3.741	87.423	3.945	82.902
5.50	.464	24.06	309.675	3.039	101.922	3.233	95.785	3.427	90.363	3.621	85.522	3.815	81.173
5.75	.450	22.80	293.459	2.954	99.342	3.140	93.458	3.326	88.231	3.511	83.582	3.697	79.377
6.00	.438	21.72	279.558	2.877	97.169	3.055	91.508	3.233	86.470	3.411	81.957	3.589	77.893
6.25	.425	20.55	264.499	2.806	94.261	2.977	88.847	3.148	84.018	3.318	79.716	3.489	75.809
6.50	.413	19.47	250.593	2.740	91.459	2.905	86.264	3.069	81.654	3.233	77.512	3.397	73.770
6.75	.403	18.57	239.014	2.679	89.221	2.838	84.216	2.996	79.777	3.154	75.781	3.312	72.166
7.00	.395	17.67	227.431	2.623	86.706	2.776	81.927	2.928	77.674	3.081	73.817	3.233	70.346
7.25	.383	16.77	215.847	2.570	83.987	2.718	79.412	2.865	75.335	3.012	71.662	3.159	68.327
7.50	.374	15.96	205.421	2.521	81.483	2.664	77.109	2.806	73.207	2.948	69.681	3.091	66.457
7.75	.365	15.15	194.996	2.475	78.786	2.613	74.625	2.751	70.881	2.889	67.492	3.026	64.440
8.00	.357	14.43	185.729	2.432	76.364	2.566	72.380	2.699	68.814	2.833	65.555	2.966	62.619
8.25	.349	13.71	176.461	2.392	73.771	2.521	69.996	2.651	66.563	2.780	63.475	2.910	60.939
8.50	.342	13.03	168.353	2.354	71.517	2.479	67.911	2.605	64.626	2.731	61.609	2.856	58.947
8.75	.335	12.45	160.244	2.313	69.130	2.440	65.673	2.562	62.546	2.684	59.703	2.806	57.107
9.00	.328	11.82	152.135	2.274	66.609	2.403	63.310	2.521	60.347	2.640	57.626	2.759	55.141
9.25	.321	11.19	144.026	2.252	63.954	2.367	60.847	2.483	58.004	2.598	55.437	2.714	53.064
9.50	.315	10.55	137.076	2.222	61.685	2.334	58.725	2.446	56.040	2.559	53.566	2.671	51.320
9.75	.309	10.11	130.126	2.193	59.336	2.302	56.527	2.412	53.949	2.521	51.612	2.631	49.458
10.00	.303	9.57	123.175	2.166	56.867	2.272	54.214	2.379	51.775	2.486	49.547	2.593	47.502

Ratio of expansion.	50 per cent.		60 per cent.		70 per cent.		80 per cent.		90 per cent.		100 per cent.	
	Total expense.	Horse-power per dollar of expense.	Total expense.	Horse-power per dollar of expense.	Total expense.	Horse-power per dollar of expense.	Total expense.	Horse-power per dollar of expense.	Total expense.	Horse-power per dollar of expense.	Total expense.	Horse-power per dollar of expense.
1.00	17.110	54.387	18.178	51.192	19.245	48.354	20.313	45.811	21.380	43.522	22.448	41.454
1.25	13.908	64.910	14.762	61.155	15.616	57.810	16.470	54.813	17.324	52.111	18.178	49.662
1.50	11.773	72.253	12.485	68.133	13.196	64.462	13.908	61.662	14.619	58.187	15.322	55.481
1.75	10.248	77.354	10.858	73.008	11.468	69.133	12.078	65.633	12.688	62.478	13.298	59.611
2.00	9.102	81.111	9.636	76.720	10.170	72.593	10.704	68.972	11.238	65.694	11.773	62.709
2.10	8.722	82.122	9.230	77.602	9.738	73.554	10.246	69.907	10.755	66.599	11.264	63.589
2.20	8.375	83.173	8.860	78.620	9.345	74.647	9.830	70.862	10.316	67.524	10.802	64.486
2.30	8.059	83.991	8.523	79.418	8.987	75.318	9.451	71.620	9.915	68.268	10.880	65.216
2.40	7.769	84.740	8.213	80.160	8.657	76.484	9.102	72.330	9.547	68.958	9.994	65.874
2.50	7.503	85.119	7.930	80.537	8.357	76.420	8.784	72.707	9.211	69.336	9.638	66.264
2.60	7.257	85.611	7.668	81.009	8.078	76.910	8.489	73.186	8.899	69.815	9.310	66.732
2.70	7.028	86.161	7.423	81.512	7.818	77.393	8.214	72.445	8.600	70.282	9.006	67.184
2.80	6.817	86.209	7.119	81.634	7.580	77.531	7.962	73.799	8.343	70.441	8.723	67.372
2.90	6.619	86.513	6.987	81.956	7.355	77.856	7.723	74.146	8.091	70.773	8.460	67.686
3.00	6.435	86.646	6.791	82.001	7.146	78.025	7.502	74.323	7.858	70.955	8.214	67.868
3.10	6.263	86.806	6.608	82.274	6.952	78.203	7.296	74.516	7.640	70.161	7.986	68.078
3.20	6.102	**86.819**	6.436	**82.313**	6.769	**78.264**	7.103	**74.584**	7.432	70.842	7.770	68.151
3.30	5.951	86.686	6.275	82.213	6.598	78.185	6.922	74.526	7.245	**71.203**	7.568	**68.181**
3.40	5.808	86.427	6.122	81.994	6.430	77.993	6.750	74.365	7.064	71.060	7.878	68.034
3.50	5.673	86.441	5.978	82.031	6.283	78.049	6.588	74.436	6.893	71.142	7.198	68.127
3.60	5.545	86.348	5.842	81.991	6.138	78.206	6.435	74.405	6.731	71.104	7.028	68.127
3.70	5.425	86.122	5.714	81.767	6.002	77.841	6.291	74.267	6.579	71.016	6.868	68.028
3.80	5.312	85.774	5.593	81.464	5.873	77.580	6.154	74.038	6.435	70.805	6.716	67.842
3.90	5.203	85.342	5.476	81.090	5.749	77.239	6.023	73.725	6.296	70.528	6.572	67.566
4.00	5.102	84.990	5.368	80.779	5.631	76.958	5.901	73.483	6.167	70.313	6.436	67.274
4.10	5.002	84.605	5.263	80.410	5.523	76.624	5.784	73.165	6.044	70.019	6.306	67.110
4.20	4.911	84.286	5.165	80.203	5.419	76.385	5.673	72.965	5.927	69.838	6.181	66.968
4.30	4.822	83.920	5.070	79.815	5.318	76.093	5.567	72.689	5.815	69.589	6.063	66.743
4.40	4.737	83.469	4.980	79.395	5.222	75.721	5.465	72.350	5.708	69.270	5.950	66.453
4.50	4.656	82.931	4.894	78.898	5.131	75.254	5.368	71.933	5.605	68.890	5.842	66.093
4.60	4.579	82.555	4.811	78.574	5.043	74.959	5.275	71.662	5.507	68.643	5.739	65.866
4.70	4.505	82.111	4.732	78.172	5.959	74.594	5.186	71.328	5.413	68.337	5.641	65.575
4.80	4.434	81.597	4.656	77.707	4.879	74.155	5.101	70.928	5.324	67.957	5.546	65.236
4.90	4.366	81.011	4.584	77.158	4.802	73.655	5.019	70.471	5.237	67.537	5.455	64.838
5.00	4.301	80.350	4.514	76.558	4.728	73.093	4.941	69.942	5.155	67.038	5.368	64.378
5.25	4.148	78.845	4.351	75.167	4.554	71.816	4.758	68.757	4.961	65.924	5.165	64.320
5.50	4.009	77.245	4.203	73.769	4.398	70.412	4.592	67.438	4.786	64.704	4.980	62.183
5.75	3.883	75.575	4.068	72.135	4.254	68.984	4.440	66.094	4.625	63.450	4.811	60.997
6.00	3.767	74.212	3.945	70.863	4.123	67.804	4.301	64.998	4.478	62.428	4.656	60.042
6.25	3.666	72.267	3.831	69.041	4.002	66.091	4.172	63.398	4.343	60.902	4.514	58.595
6.50	3.562	70.353	3.726	67.227	3.890	64.421	4.054	61.815	4.219	59.397	4.383	57.174
6.75	3.470	68.880	3.628	65.880	3.787	63.114	3.945	60.580	4.103	58.253	4.261	56.093
7.00	3.386	67.168	3.538	64.281	3.691	61.617	3.843	59.180	3.996	56.914	4.148	54.828
7.25	3.307	65.266	3.454	62.491	3.606	59.940	3.748	57.589	3.896	55.459	4.043	53.387
7.50	3.233	63.538	3.375	60.865	3.518	58.391	3.660	56.125	3.802	54.029	3.945	52.071
7.75	3.164	61.629	2.302	59.053	3.440	56.684	3.577	54.513	3.715	52.489	3.853	50.008
8.00	3.100	59.912	3.233	57.447	3.366	55.177	3.500	53.065	3.663	51.122	3.767	49.304
8.25	3.039	58.065	3.164	55.701	3.298	53.535	3.427	51.491	3.556	49.623	3.686	47.873
8.50	2.982	56.456	3.107	54.185	3.233	52.073	3.358	50.134	3.484	48.321	3.610	46.635
8.75	2.928	54.728	3.050	52.539	3.172	50.518	3.294	48.647	3.416	46.909	3.538	45.292
9.00	2.877	52.879	2.996	50.779	3.114	48.855	3.233	47.056	3.352	45.386	3.470	43.842
9.25	2.829	50.910	2.944	48.921	3.060	47.067	3.175	45.362	3.291	43.763	2.406	42.385
9.50	2.784	49.237	2.896	47.332	3.008	45.570	3.121	43.917	3.233	42.399	3.345	40.979
9.75	2.740	47.491	2.850	45.458	2.959	53.976	3.069	42.400	3.178	40.945	3.288	39.572
10.00	2.699	45.637	2.806	43.817	2.913	42.284	3.020	40.786	3.126	39.403	3.233	38.059

sion, if it has any effect at all, tends to lower the ratio of expansion to be adopted, necessitating following further.

From the figures in the table it will also be noticed that, when the best point of cut-off for average conditions of running is determined, the point of cut-off can change during running, to meet necessary fluctuations of power of 10 per cent., or even slightly more, on each side of the average or usual power required, without at all sensibly affecting the actual economy of the steam engine.

DISCUSSION.

Mr. Kent: I regret that I have not had an opportunity of studying Messrs. Wolff and Denton's original paper, or to look over this one. But, from a hasty glance at their former paper, it strikes me that there is some fallacy in it. While we may admit that they have solved the problem for some particular engine, and that its best ratio of expansion is six, or less than that, they have not solved the question which is the best for us to employ to obtain any given horse-power. Suppose we have four engines of the same kind which, expanding six times, would give horse-powers, one, two, three, and four hundred. They all, according to Messrs. Wolff and Denton's theory, would give the most economical effect at six expansions; but the actual economy of the 400 horse-power engine would be far greater than the 100 horse-power engine, one reason for this being that it would not require any greater amount of engineer force, and, perhaps, for some other reasons. For the purpose of obtaining 200 horse-power, I think it possible that it might be found more economical to use the 300 or the 400 horse-power engine, with a higher rate of expansion than six (that of their maximum efficiency) than to use the 200 horse-power engine cutting off at its point of maximum efficiency. Is the most economical point of cut-off of any given engine the same as the most economical point of cut-off to secure a given horse-power? I think that Messrs. Wolff and Denton's paper, so far as I can see, does not solve the question.

Mr. Porter: Until a recent period the practice has been to overdo expansion. I suppose that there is now no question that it is capable of being demonstrated very clearly, without any reference whatever to the matter of condensation in the cylinder, that a non-condensing engine which cuts off so early as a quarter of the stroke cuts off too early with eighty pounds pressure of steam for

the most economical results; and, undoubtedly, when all things are considered, the engine should be no larger than is necessary to do the work when eighty pounds of steam are following a full one-third of the stroke, and if I understand the general drift of the argument presented in these two papers, it is decidedly in that direction. In adapting an engine to a particular work I fancy that engine-builders nowadays look out sharply, and do not cut off too early, if they desire to bring themselves credit. But, of course, the test has always been how little coal we can burn, and other items of expense have, to some extent, been overlooked.

The cost of attendance would be varied much by the size of the engine. A very large engine, cutting off very early, might call for an accomplished engineer to sit by it all the time; while a smaller engine, cutting off later in the stroke and doing the same work, would be too insignificant to require such an ornamental appendage. The tendency, therefore, is to dispense with the services of that gentleman in ordinary cases. For myself I am heartily in sympathy with what I understand to be the aim of the gentlemen, on the basis on which they make their calculations. As regards the general tendency to substitute as small an engine as possible, we have gone through some changes in that respect, as I understand it. The builders of cut-off engines originally, when they began to be made, employed what was then considered high piston speed in small engines, but they soon found the limit that could not be passed with any satisfaction on account of the character of the mechanism. They soon changed their ground and preferred to sell large engines at large prices, instead of small engines at smaller prices. The original effort to employ rapid speeds was abandoned. From a commercial point of view it was altogether for their advantage to do so and to cut off early, and there was a tendency to magnify the importance of early cut-off, and to overlook the evil attending early cut off, but I think we have got past that now, and well past it. I think that engines that cut off very early and are put in intentionally for that purpose will be very few and far between in the future.

Professor Robinson: Say a company starts manufacturing. They want the steam power for the least money. They do not care whether the engine is large or small. The way to solve that problem does not seem to me to be to take one particular engine and find the most economical point of cut-off in that, but to find what engine will do this work most economically. We may first

find the point of cut-off in a certain engine, if you please, that will do this work, and then find the point of cut-off in another engine to do this same work; thus figuring up the expenses for a number of dimensions of engines, boilers, etc., taking account of the money invested in the engine, and in fuel and oil. Then take the smallest cost for doing this work, from among all these values, and thus get the most economical point of cut-off, and that engine is the one to do the work for the establishment.

Mr. Kent: The engine selected by Professor Robinson might not be running at its most economical point of cut-off.

Professor Robinson: Yes, sir, I believe it would, for that particular establishment.

Mr. Woodbury: The other day this question of the economy of power was given to me. The treasurer of a mill operated by water-power wished to anticipate a cessation of the operation of the mill in case of drought, by putting in an engine. He came and asked me about it, and I inquired, "Do you prefer a high-pressure or a condensing engine?" He did not know. I made some estimates for the size of engine for the power he would probably want. A condensing engine would cost about $1200 more than a non-condensing engine, and it would cost about $300 extra to lay a pipe and make the connections with the condenser. This amounted to $1500, the annual interest on which, at five per cent., was $75. For repairs to the condenser, for oil and renewal of valves, I made an estimate of $30, making the annual cost of that condenser $105. The difference in estimated running expenses of coal alone, between a condensing and a non-condensing engine, working at the assumed cut-off with coal at about $7 a ton, which is the cost at those mills, was $1.40 a day, and the problem then resolved itself into this statement. A condensing engine will cost $105 a year more for interest and repairs, but it can be operated for $1.40 a day less. Divide $105 by $1.40 and you get 75 days. If you are going to run your engine more than 75 days a year it would be cheaper to have a condensing engine; if less than 75 days, a non-condensing engine.

XLI.

COMPARISON BETWEEN DIFFERENT TYPES OF ENGINES.

BY CHARLES A. HAGUE, CHICAGO, ILL.

IN this paper it is proposed to draw a comparison between single cylinder engines and different types of compound engines, for the most part by theoretical methods, and with reference mainly to the important features of cylinder temperature and the distribution of pressure throughout the stroke. It is not the purpose herein to state any definite conclusions, but rather to invite discussion on the subject, with the view of obtaining expressions calculated to indicate to what extent the adaptations of the means to the ends have been studied in applying either type of engine.

Those theories which are confirmed by experience, and which are derived from the appearance of phenomena, form the basis for the explanation of facts. When we fail to explain an observed fact through theories heretofore held, we must remodel our ideas. When phenomena come under our notice which we could confidently expect according to our view of things, our theories are established and our confidence in them is strengthened, because they are supported by experience. If, on the other hand, an occurrence takes place which, to our way of thinking, was impossible and unlooked for, we must overhaul our theory in order that the fact which we have observed and which we cannot deny shall agree with our so-called "laws of nature."

This system of remodelling and completing a logical chain of ideas in the department of steam-engineering has been most marked; and there is evidence at hand that there is room for *some* remodelling to be done in the future.

The development of the higher grades of expansion has resulted in a great increase in certain losses that were not very serious with the lower grades; and we are thus admonished that the theory of gain by expansion will not be complete, and hence not fully successful, unless we can in some manner discover the entire sequence of facts in the case, and thereby shape our efforts to correspond.

The different branches of steam-engineering deal with different demands.

In mill practice, by the use of one or more independent cylin-

ders, and a high rate of rotation, the distribution of energy throughout the stroke, even with the very high grade of expansion herein indicated, might be satisfactorily effected; but the extreme variation of cylinder temperature presents a serious obstacle to the most economical operation. The cooling effects of expansion and exhaust, are supposed to be somewhat mitigated, however, by the shortness of the periods of time during which the interior cylinder walls are exposed, when a high rate of rotation is employed; in such an engine, where the steam chest extends the length of the cylinder, and steam-jackets that side as it were, a steady induction of heat assists in preserving the cylinder temperature, especially if the steam is 30° or 40° superheated. The extent to which it pays to carry expansion is a question depending largely upon the work to be done, and upon the restrictions surrounding the case in hand. The practical gain exhibited by the higher above the results of moderate practice is comparatively limited, when the moderate expansions are performed under conditions peculiarly adapted to secure the benefits of all possible advantages. For example, it is quite possible to develop a duty of ninety million foot-pounds from a single cylinder condensing engine 18″ × 36″, with an expansion of five volumes. Many engineers would feel quite proud of a pumping-engine capable of reliably performing ninety millions duty, massive though it might be, in proportion to the 18″ × 36″; perhaps developing no more power than the latter, and even though its first cost might be much more per horse-power.

With marine screw-engines, where some certain diameter and pitch of wheel is desired, and a certain number of revolutions demanded to obtain the best results, the absence of a rate of rotation sufficiently high to effect a proper distribution of energy at high grades of expansion, together with the moderate variation of temperature in each cylinder, leads to compounding.

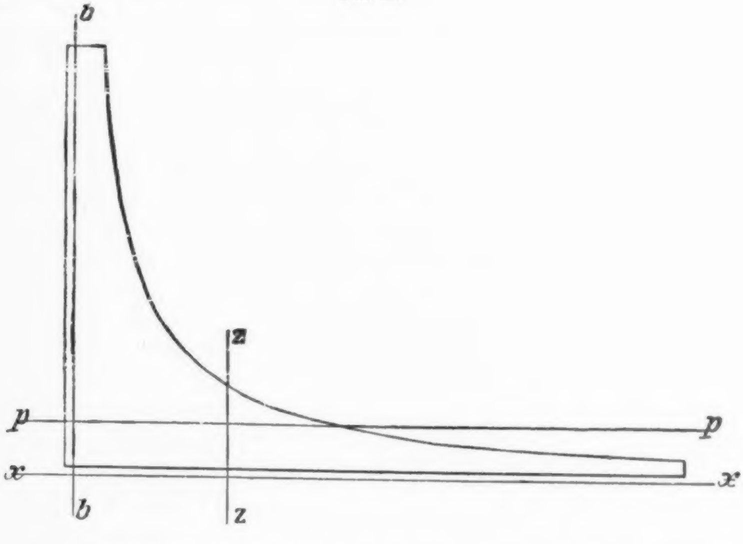
In attempting high grades of expansion in the cylinders of pumping-engines, the difficulties are vastly enhanced by the unyielding, inelastic qualities of the fluid upon which the energy is exerted. The task here being to construct a machine, in one end of which is to be employed a highly elastic fluid, demanding quick motion; while in the other end is to be operated upon a fluid extremely inelastic and demanding to be moved at a slow and uniform rate of speed. The Cornish engine is peculiarly adapted to meet the difficulties of unequal pressure throughout

the stroke, inasmuch as the great variation in energy is expended in lifting the massive pump-rods during the suction stroke of the pump; this engine is also as peculiarly adapted to nothing else but the removal of water from deep mines, the purpose for which it was originally designed.

The attempt to adapt it to city water-works pumping has, to be brief, been a very costly experiment, not only in its own failure, but in the shortcomings of the numerous modifications of itself, which owe their being to the desire to obviate the apparent disadvantages under which it labored. The Cornish engine and its offsprings are prominent examples of pumping-engines in which high expansion has been carried on in single cylinders.

Examples of the distribution of pressure throughout the stroke under high grades of expansion in a single cylinder, and also in different types of compound engines, are exhibited by the accompanying diagrams.

FIG. 88.



The curves are theoretical; calculated from relative volumes. They would be somewhat distorted in practice by the transmission of heat, angularity of connecting-rods, etc.; but the distortion would very likely operate to raise the terminal pressures, and thereby effect a still more nearly uniform distribution in all cases, so that the comparisons would be about the same.

Fig. 88 represents expansion to twenty volumes in a single cylin-

der of 42″ diam. The initial pressure is 120 lbs. absolute, the terminal 6 lbs. *pp* is the atmospheric line, *bb* the admission line, and the line at the extreme left indicates the volume of waste room reduced to terms of the stroke. The ordinate *zz* represents a point in the stroke at which the mean effective pressure is reached; the counter-pressure above the total vacuum is 2 lbs.

The principal features of Fig. 88 are as follows:

Mean effective pressure, ...	22 lbs.
Greatest " "	118 "
Least " "	4 "
Excess of greatest pressure above mean, . .	436 per cent.
Deficiency of least pressure below mean, . .	81 " "
Highest temperature of steam, ...	344°
Lowest " " (terminal), . .	170°
Variation from initial to release, ...	174°
Mean total effect on the piston, ...	30,470 lbs.
Greatest " " "	117,800 "
Least " " "	6,200 "

FIG. 89.

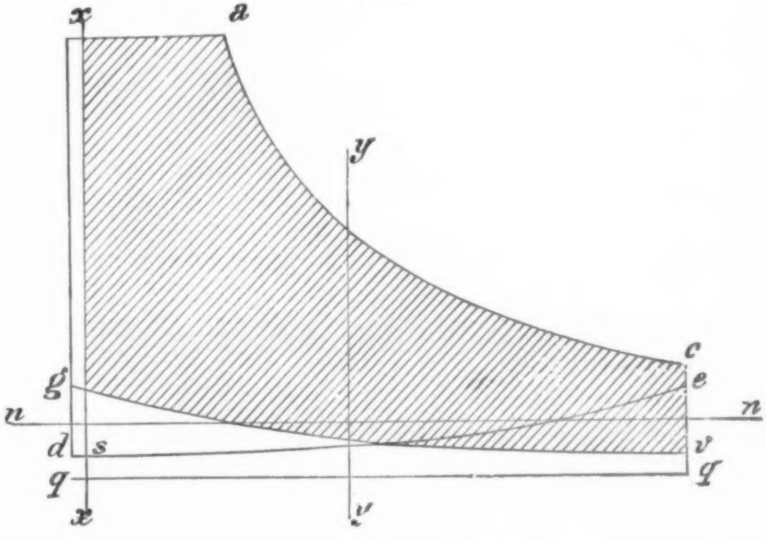
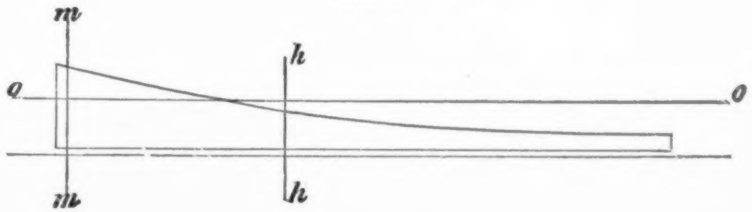


FIG. 90.



Figs. 89 and 89 represent diagrams from a compound engine. The cylinders are supposed to be connected by short passages from each end, and the pistons working in opposite directions as though the connections were made to diametrically opposite crank-pins. It will be observed that the terminal pressure in the large cylinder is the same as in the single cylinder of the previous example; and as the cylinders are of the same dimensions, that is, this larger one of the compound and the single one, it is fair to presume that the consumption of steam will be alike in both cases.

The high-pressure cylinder is 20″ diam., and the low-pressure one 42″ diam. The exhibit of Figs. 89 and 90 is as follows:

Small Cylinder (20″).

Initial pressure (absolute),	120	lbs.
Grade of expansion,	4	volumes.
Mean effective pressure,	$56\frac{88}{100}$	lbs.
Greatest " "	105	"
Least " "	24	"
Mean total effect,	17,860	"
Greatest " "	22,970	"
Least " "	7,536	"
Excess of greatest pressure above mean,	84	per cent.
Deficiency of least pressure below mean	57	" "
Highest temperature of steam,	344°	
Lowest " "	250°	
Variation from initial to terminal,	94°	

Large Cylinder (42″).

Initial pressure (absolute),	25	lbs.
Grade of expansion,	$4\frac{16}{100}$	volumes.
Mean effective pressure,	$12\frac{52}{100}$	lbs.
Greatest " "	23	"
Least " "	4	"
Mean total effect,	17.349	"
Greatest " "	31,855	"
Least " "	5.540	"
Excess of greatest pressure above mean,	84	per cent.
Deficiency of least pressure below mean,	68	" "
Highest temperature of steam,	240°	
Lowest " "	170°	
Variation from initial to terminal,	70°	

Summary of Compound Engine.

Mean total effect on both pistons,	35,200	lbs.
Greatest " " "	64,825	"
Least " " "	13,076	"
Excess of greatest effect above mean,	84	per cent
Deficiency of least effect below mean,	60	" "

When the release-valve on the high-pressure cylinder is opened the pressure falls slightly as the steam expands into the passage, and as the expansion continues in the large cylinder the line drawn during the return stroke of the small piston is from *e* to *d*, so that the figure as drawn by the indicator pencil would be that bounded by *x a c e s*; but the line *e s* is the counter-pressure that resists the driving pressure on the opposite side of the piston, and hence the diagram of actual pressure would be formed by reversing the line

Fig. 91.

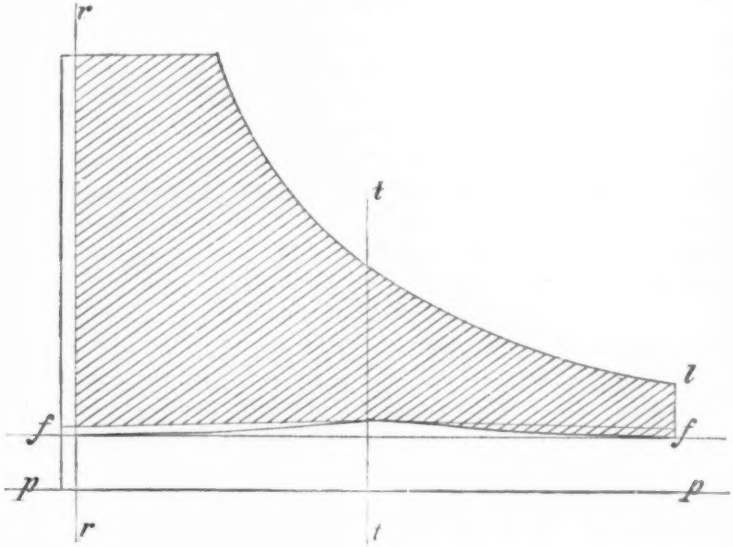
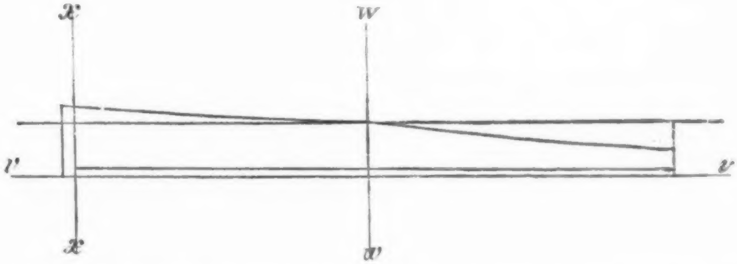


Fig. 92.



e s to the position shown by the line *g v*, and is represented in Fig. 89 by the shading.

Figs. 91 and 92 represent diagrams from a compound engine, with cranks at right angles, and with a receiver between the cylin-

ders of five times the capacity of the small cylinder. The diameters of the cylinders are 20″ and 40″.

When the terminal of 30 lbs. absolute is reached at l in Fig. 91, communication is established with the receiver and the pressure falls to 17½ lbs., an average resulting from the union of the receiver containing a pressure of 15 and the cylinder containing a pressure of 30 lbs. At the initial pressure and grade of expansion in the small cylinder here given, the pressure in the receiver gradually rises to 15 lbs., after which the abstraction of steam by the low-pressure cylinder prevents further rise.

As the small piston returns to about half-stroke before the induction-valve of the large cylinder opens, the steam in the receiver is compressed to about 19 lbs. At this point steam is admitted to the large cylinder, and the increase in volume, as the large piston progresses, reduces the pressure again to 15 lbs. just before half-stroke is reached; the cut-off now takes place, and the receiver is left with 15 as before. The distribution of energy throughout the stroke, and the variation of temperature, differs but little from Figs. 89 and 90.

XLII.

ROLLED CAST-STEEL CAR-WHEELS.

BY JACOB REESE, PITTSBURGH, PA.

THE great majority of car-wheels used in the United States are made of chilled cast iron, and possess a close, dense, hard wearing surface, with a softer and less dense internal structure. This kind of structure not only gives the best wearing-power, but also, by reason of its softer center, imparts toughness and ability to endure expansion and contraction incident to the continual shifting load and the accompanying shocks.

I have designed a process and machinery by which I am enabled to produce a steel car-wheel possessing the same relative texture in its different sections as is exhibited in the chilled cast-iron wheel, which has proven so serviceable in railroad practice in the past.

Attempts have been made heretofore to secure in a steel car-wheel those peculiar properties possessed by the chilled cast-iron wheel, but, as far as I am aware, the object has not been fully secured.

In the manufacture of steel car-wheels various difficulties have presented themselves. It has been impossible to produce a solid steel casting free from air-cells, blow-holes, or porosity, unless the metal was highly charged with silicon and carbon. Such castings required to be annealed before use, which was an expensive and tedious operation, and it was necessary to subject the wheel to a subsequent hardening operation if it was desired to secure the same relative physical properties as those possessed by the chilled cast-iron wheel. The presence of silicon in any great degree is highly objectionable; for, although it serves as a hardener, it does so at the expense of the strength of the metal, as it imparts brittleness and adds no tempering qualities. Wheels made by this process are liable to crack in tempering, and, owing to the internal strains produced thereby, are liable to break without notice when in use.

Various methods have been devised to obviate these difficulties. What is known as the paper car-wheel is composed of a rolled steel tire, a metal hub, and an intermediate filling of compressed paper, held in position between two annular plates secured to the tire and hub. This wheel possesses, in a high degree, the characteristic properties of the chilled cast-iron wheel, and has a greater wearing power; but, owing to the large amount of labor involved in its construction, it is entirely too expensive to come into general use for freight traffic. Wheels have also been made by rolling a cast-iron tire to the required form and then shrinking it into a cast-iron centre, or by heating the steel tire to a high degree and casting a cast-iron centre thereto; but both those methods involve considerable labor, and it is difficult to produce good sound wheels by their use.

It has also been proposed to manufacture solid steel car-wheels by compressing the metal while in a fluid condition; and also by casting a wheel blank, and then to compress it by rolling or hammering its entire surface to reduce its porosity and secure a solid structure. Neither of these methods produces an article having the peculiar properties of the cast-iron wheel, as the metal is of uniform density and structure throughout.

Now, in the use of my improvement, which is designed to overcome the objectionable features of such manufacture, and to produce a car-wheel having a tough elastic centre and a perfectly true tread, of a close, firm, and even texture, I cast the wheel-blanks of the same general conformation, but of smaller diameter and thicker in the flange and outer part of the web than an ordinary

wheel. The steel used for this purpose must be cast steel, of a suitable quality to produce as solid a casting as possible, and I propose to produce it by the following method:

I first decarburize and desiliconize the cast iron, as in the ordinary open-hearth practice, and then charge the metal with sufficient silicious pig to reduce the gaseous and solid oxides and leave a residuum of .050 per cent. of silicon in the bath. The object of this step is to remove the oxygen by the action of the silicon, and to retain a sufficient amount of silicon in the bath to protect the carbon from oxidation during the treatment of the metal for the next thirty minutes. When the ebullition resulting from the reduction of the gaseous oxides ceases the metal is charged with sufficient spiegel to increase the carbon to the desired degree. The presence of the silicon protects the carbon from oxidation, and the metal may be held in a state of rest and at a condition of high fluidity while it is subjected to a dead melt for about thirty minutes after recarburization, during which time the occluded gases escape and the silicon is reduced to a low degree; so that a solid homogeneous casting, low in silicon, free from blow-holes, air-cells, and containing a definite amount of carbon, may be produced.

These wheel-blanks are cast either in sand or metallic moulds, as may be desired; and, when the blanks are produced, they are taken, one at a time, while still hot—or they may be allowed to cool and be subsequently reheated—and inserted between the dies of my machine, and treated in the manner which I shall hereinafter describe.

I shall now describe the construction and mode of operation of the machine which I use for compressing, truing, and straightening the blanks.

In the drawings on accompanying plate, Fig. 93 indicates a side elevation, partly in section, of an improved machine adapted to the use of my invention, showing a sectional view of the pillow blocks, the revolving dies, and of a car-wheel engaged between the said dies. Fig. 94 indicates a ground plan of the machine. Fig. 95 indicates a sectional view of a finished car-wheel. The dotted lines represent the unfinished blank in the form in which it is cast before finishing, showing a greater thickness of flange, tread, and outer part of the web than in the finished wheel.

A indicates the bed-plate.

B, B^1, B^2, B^3, and B^4 are the pillow blocks in which the shafts are journalled.

C indicates the main driving shaft, mounted in the pillow-blocks B and B^1. On this shaft is mounted a roll, E, which has a working face of a shape to correspond to the shape of the tread of the car-wheel to be produced. This shaft C is also provided with a gear-wheel D, which meshes into and communicates motion to the gear-wheel D^1, mounted on the shaft C^1. A rolling die, F, having a shape to correspond to the shape to be given to the inner side of the wheel to be produced, is attached to one end of this shaft.

C^2 indicates a shaft mounted in the pillow-block B^4, and is provided with a die having a working surface of a shape to correspond to the shape to be imparted to the opposite of the wheel. This shaft C^2 has no connection with the shafts C and C^1, and rotates only through the frictional contact of the wheel-blank between the dies F^1 and F.

H indicates an eccentric pawl, which engages in a slot in the die F^1, and is withdrawn from or thrown into the slot by means of a hand lever, not shown, to prevent, or admit of, rotation of the die F^1, as may be desired, during the rolling and compressing of the blank.

G indicates a direct-acting hydraulic ram for forcing the die F^1 against the blank confined between it and the die F.

G indicates clutches, which are attached to the end of the plunger of the ram, and engage in an annular groove or slot in the shaft C^2, in order to communicate a positive backward movement to the shaft C^2 and die F, to admit the insertion of a fresh blank into the machine, and also to allow the rotation of the shaft C^2 and die F^1, when the eccentric pawl is relieved from the slotted part of the die F^1.

The operation of the machine is as follows: The die F^1 is drawn back by the plunger to admit the insertion of a blank between it and the die F, and is locked, to prevent its rotation by throwing the eccentric pawl H into its slotted portion. Power is then applied to the shaft C, causing it to rotate, together with the roll E and the pinion D, which communicates the motion to the pinion D^1, and from it to the shaft C^1 and the rolling die F. One of the heated blanks is then inserted into the machine between the rolling dies F and F^1, and the die F^1 is gradually forced, by the action of the hydraulic ram, against the wheel-blank until it is reduced in thickness and increased in diameter sufficiently, so that the metal is forced against the edge of the roll E, and the blank is

almost brought to the required form. During this part of the operation the blank, being held by the die F^1, will not rotate, or will remain nearly motionless, until the metal is forced out against or in frictional contact with the face of the roll E, at which time the blank will rotate. Thus at first the die F moves over the face of the wheel, truing it and forcing the metal out against the face of the roll E, and then the blank will rotate upon the die F^1. Thus the blank is smoothed, trued, and rolled, first on one side by the movement of the die F, and then trued and straightened on its opposite side by its movement over the stationary face of the die F^1, and both these movements assist the action of the direct pressure in forcing the metal out against the action of the roll E, and aid in forming a true and perfect face to the flange and tread of the wheel. At this point, when the blank is compressed almost to the thickness required in the finished wheel, the pressure is relieved, and the eccentric pawl H is thrown out of gear to allow the die F^1 to rotate, when the pressure is again applied, and the blank is reduced and brought to the exact form desired.

After the wheel has been brought to the required shape it may be bored, and if it contains sufficient carbon to impart to it the required degree of hardness it will be ready for use without further treatment; but if it is desired to make a wheel of soft steel, low in carbon, it should be tempered in an oil-bath, or the tread and flange should be tempered in oil or water.

The wheel produced by this method may be readily distinguished, by its appearance, from all steel car-wheels heretofore produced, as its surface is marked by a series of circular lines extending around the sides of the wheel from its centre to its periphery; and, when the wheel is broken, the physical structure of the metal may be readily distinguished by its gradually increasing density or closeness of texture from its centre to the face of the tread, and the wheel may also be distinguished by its perfect smoothness, roundness, and its freedom from air-cells, blow-holes, and other imperfections which characterize ordinary cast-steel car-wheels.

One of the advantages of a wheel made by my method is that it possesses a circular texture, the texture of the metal being in the line of travel of the wheel, while in all other solid cast-steel car-wheels the texture runs in radial lines from their axes to peripheries, and the metal has a tendency to crack in such lines. By the use of a plant of sufficient capacity to insure economy, my

improved rolled cast-steel car-wheel may be produced at a cost not greatly exceeding that of steel rails.

DISCUSSION.

MR. LAUREAU: I notice that it is said there that no steel car-wheel has been made successfully—that the object has not been fully carried out. I want to say that at Bochum in Germany, I saw a steel car-wheel which had just come out of the casting-house, and the number stamped upon it was considerably over 100,000; and at Krupp's they have made fully the same number, and the manufacture is carried on very successfully all the time. So that cast-steel car-wheels are not by any means very hard to make. It is quite a successful manufacture. Of course this steel contains some silicon, but not a very large quantity; about, I should say, three-tenths of one per cent. is the limit. They find no difficulty in making them so that they will not break. They hardly know an instance where a steel cast wheel has broken. They are cast with a thick rim so that they can be turned two or three times. The only difficulty is in turning them after they have been on the road for some time. It hardens the outside to such an extent that they have to be ground with emery. It makes a rather difficult job, because the sparks fly all around, and it is hard to see when it is ready to be taken off the tool. Nevertheless it is done successfully.

MR. DURFEE: A wheel was exhibited in the Belgian department of the Centennial Exhibition which I was informed was manufactured by some rolling process. I was told that the machine had two sets of rolls working on opposite surfaces of the blank, and that their axles were parallel with a radial line of the wheel. The rolls were conoidal in form and were attached to horizontal rotating shafts. I was told that quite a number of them had been made; and as a matter of information I should like to ask if any one here can say whether such a wheel as is described in this paper has been manufactured.

DR. GRIMSHAW: I think that the wheel was made at the Cockerill works. I know they had a large establishment for making the tires from cheese-shaped blooms.

MR. LAUREAU: I do not know whether anybody here can answer the question I am about to ask. What is the difference, I would like to ask, between the process that Mr. Reese describes in making the first blank, and what is known as the Terre Noire

process of making steel castings? I think it must be merely a modification of it, and a modification which I think would not work very well. In the operation, the bath is first decarburized and desiliconized, and if you decarburize and desiliconize entirely you are bound to oxidize somewhat, and to such an extent that .05 of silicon only in a bath would not protect the iron and carbon from oxidation.

DR. DUDLEY: The question suggests itself to my mind whether the metal would not get so cold during the operation that nothing could be done with it. I would inquire of somebody whether it has ever been practically done, or whether it is merely a scheme on paper.

DR. GRIMSHAW: What becomes of that hole in the hub?

MR. MORGAN: I think practical men would find a means of getting the metal out.

DR. GRIMSHAW: I would like to inquire as to whether the hole in the hub is made larger than finally necessary, with the expectation of having the metal crowded in to bring it to a smaller diameter.

MR. REESE: I designed to cast the hole in the hub over a pipe —wrought iron or steel pipe—so as to get a perfectly solid opening. I find in my experience in making steel castings, that there is a good deal of difficulty in getting a solid hole in the centre of the casting, and by casting it on a pipe, heating the pipe, so as to make what is termed a teeming weld, a perfectly solid casting is secured; not only a perfect hole, but the metal adjoining the pipe seems to be more solid than it would otherwise be with a sand core. The hub was intended to be a little thicker through, so as to be condensed somewhat on the pin. The main work on the wheel in rolling will be on the inside curve of the tread. The dies, in coming in contact with the metal, press the metal on the inside curve of the tread, so as to push it outward. The machinery is also not entirely in a straight line, but is a little oblique, so that the pressure comes mostly opposite the outside roll, and is not uniform all round the web, or all round the wheel, until it has been thrown out to its full diameter. After it has been thrown out the machinery is then straightened up.

MR. DURFEE: I would like to repeat a question I asked before. To what extent has this manufacture advanced? How many wheels have been made by this method?

MR. REESE: There have been none made by me by this pro-

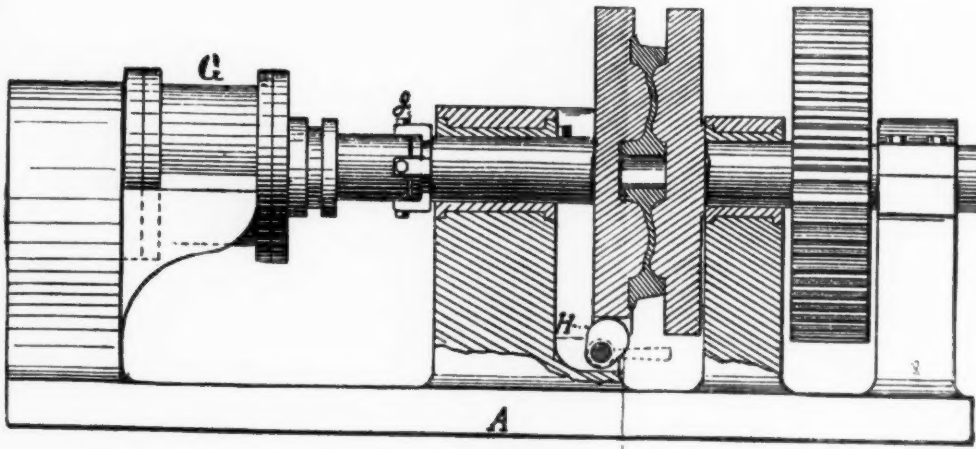


Fig. 93.

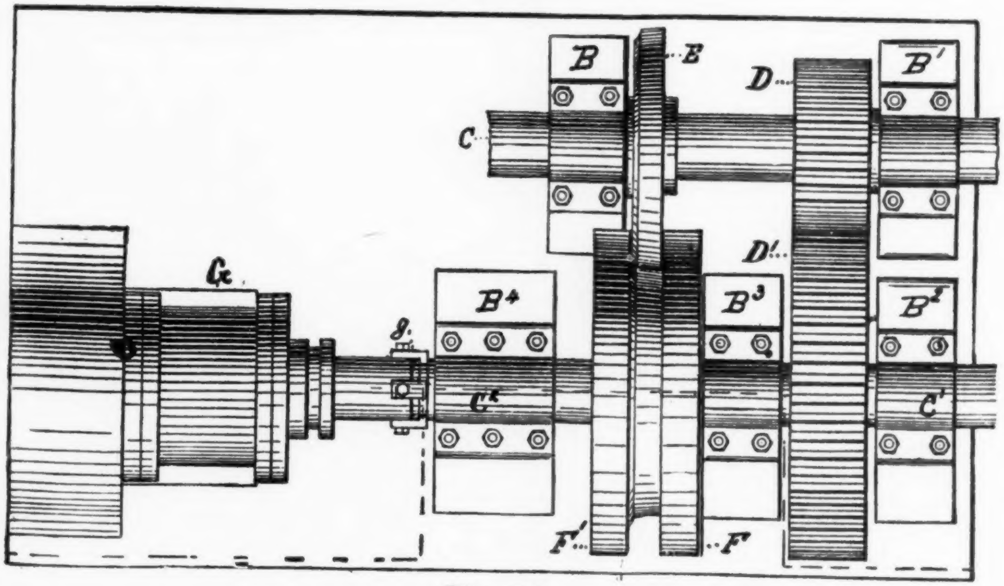


Fig. 94.

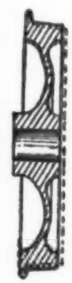


Fig. 95.

cess. It is a new idea. The patents for it were pending when this paper was offered. They have since that been issued. I have not means of testing the matter myself, and have not yet been able to get other parties to do so.

MR. LAUREAU: About the matter of manufacture—the remark I made on that subject was perhaps a little foreign to the Mechanical Engineers' Society; it would have been more appropriate in a meeting of the Mining Engineers. But I question whether that small amount of silicon will prevent the formation of carbonic oxide in a sufficient quantity to prevent the formation of blowholes. If a small amount of silicon only is placed at the end of the decarburizing operation, the greater portion of the silicon will be used in taking up the oxidation in the same way as manganese does; and the question is whether there would be silicon enough left in the bath then to prevent the formation of carbonic oxide.

MR. REESE: In regard to the matter of silicon, I think the gentleman is mistaken. My experience is that when metal is melted in a cupola both the silicon and carbon are reduced nearly .50 per cent. In some of my experiments they ran from .435—none lower than .435—to .50 per cent. I find then, after the metal has assumed a state of rest in an open-hearth furnace with a silicious bottom, that what is known as the silicon period takes place, and during that silicon period there is no elimination or loss of carbon, whatever be its quantity, when a perfect state of rest is secured. No ebullition from the oxidation of the carbon takes place until the silicon is reduced to .020—I have made careful experiments, and I never found it to exceed .025. I say when a perfect state of rest is secured, because there may be elements present in the metal which will cause ebullition other than carbon. Then the elimination of carbon would commence in a small degree before the silicon has been reduced to so low a point. Now in order to produce castings low in manganese, or with a fixed and definite amount of manganese, I prefer to deoxidize the metal with silicon —with a silicious pig—and I think you will find in looking over the paper, that I do not provide sufficient silicon to deoxidize the metal alone, but that there is somewhat of an excess, and that I then hold the metal in a state of rest. I charge it with more silicon than is necessary to deoxidize it, so as to raise the amount of silicon above .020 in a sufficient degree, so that a state of rest can be secured for thirty minutes without the elimination of carbon. Now we have by experiment and calculation—I cannot give it to

you from memory—arrived at the quantity of silicon necessary for each minute of the duration of this state of rest. Consequently if I want a state of rest for thirty minutes, I put in sufficient silicon to reduce the oxides and carry over sufficient to hold the metal in a state of rest for those thirty minutes; and if ebullition commences by putting in additional silicon it is made to cease instantly, until the silicon runs down to .020. In regard to getting rid of the carbonic oxide—this porosity—it is now a pretty well-established fact that there is very little carbonic oxide in the metal, and that the porosity of the metal is largely due to hydrogen. Experiments recently made on the other side prove that hydrogen is the principal cause of porosity in steel, and by using sufficient silicon to hold the metal in a state of rest, the hydrogen escapes, and as the hydrogen disappears the pores are closed and the metal assumes a close homogeneous mass. I cannot reason from theory why it should be otherwise than it is in practice. In my practice in making steel castings, of which I have made a good many, I found that I could secure as close a casting when the silicon had run down to the lowest point as I could with .75, although I never used so much of it as that, practically.

XLIII.

FIRE PROTECTION OF MILLS.

BY C. J. H. WOODBURY, BOSTON, MASS.

The reduction of taxation furthers prosperity, by adding to the accumulations of industry, and increasing the capital in a community.

The fire tax is the heaviest single tax in this country; the cost of fire insurance and fire departments together, has amounted to over one hundred and twenty-five million dollars, annually, during the last five years. If the losses can be decreased, the cost of insurance will diminish in like ratio.

Most of this vast sum is the price paid for incompetent management, and hazardous construction, and the means for its diminution are wholly within individual control.

The question of defence against fire obtains but little attention from those in charge of property, the whole matter being abandoned to the public fire departments, in a manner that is without

parallel in shifting the other responsibilities of business. This is, indeed, a tribute to the efficiency of fire departments; and, were not that portion of our local governments administered with extraordinary ability, our cities would constantly be ravaged by conflagrations.

The owner is as truly the natural defender of his property against fire as against theft. This principle is incorporated in the statute law of France.

The prompt intelligent effort of occupants will accomplish more in subduing a fire than the endeavors of brave firemen summoned after a delay. The endeavors of the firemen are too often futile, because the hollowness and sham of general construction embodies every device and arrangement to secure complete destruction whenever a fire begins its consuming course.

The writer does not assume to present a treatise on the subject of mill protection, but merely to offer, in a few plain words, some results of experience in the protection of manufacturing property, which have not yet crystallized into books.

In briefly citing reasons for conditions, either essential or pernicious, in the fire system of a mill, about every statement could be emphasized by the history of burned mills, whose destruction was due to these apparently trivial causes.

As the only value which this article can possess is in the application of its precepts, the names of the apparatus referred to are given, not because they are perfect, or that others may not have equal merit, but they have proved to be reliable in actual operation.

The arrangements for the defence of property against losses by fire, may be classified in four divisions. The reader will probably assume that water is the principal division, or even the alpha and omega of our creed; on the contrary, water is only a *detail.*

First: The chief consideration consists in anticipating all preventable causes of fires, originated by neglect of any kind. There are but few fires which could not have been prevented by due foresight on the part of some one; that person may be an humble subordinate, the superintendent on the premises, or others higher in authority, such as the financial heads of the establishment, but the responsibility for the disaster rests upon some one.

Other fires owe their existence to causes so far beyond the ordinary control of those intrusted with the management, that they may be considered for all practical purposes, unpreventable.

The second division consists in preparation of the methods of fighting fires, by fire-organization of the men.

The third clause refers to the question of water supply for fire purposes, and the best apparatus for the protection of mills, giving the results of experience in the arrangement of fire service.

The fourth comprises those elements of construction and arrangement of buildings that offer the most efficient means of retarding the spread of the fire; the aim being, that the limits of the destruction shall be reduced to a minimum by making buildings slow-burning, rather than striving to make them fire-proof. A fire-proof mill is a commercial impossibility.

An analysis of the records of the Boston Manufacturers' Mutual Fire Insurance Company, covering $630,000,000 of risks during a period of thirty years, warrants the statement that the greater losses are due to the failure of the fire apparatus at the critical moment, and not to the absence of a suitable equipment.

It is, indeed, an exceptional fire that could not have been extinguished in its earlier stages, if the means at hand had been used with intelligence and energy. As in every other crisis of life, organization is superior to random effort. The thinking must be done beforehand. It is bad generalship to form the plan of battle in presence of the enemy.

The value of the best apparatus is limited by the competency with which it is managed; and is generally worthless, except when its use is directed by the wise cool head of a leader; circumstances may deprive one of much that is desirable or seems even necessary, but nothing will atone for the absence of some directing mind. In the lack of such management any fire apparatus is a delusion and a snare.

Perhaps it may seem as if this was an effort to prove undisputed facts, but I have seen many men who regarded their fire systems as a heathen does his household god, as a fetich able to charm away the fire. The greatest losses paid by the Mill Insurance Company referred to might have been diminished by the exercise of ample foresight in the care and use of apparatus.

Plans of Organization.

There are numerous plans of fire organization, and the details differ with the conditions and administration of each establishment, but this principle is the salient point in all. Giving in-

struction in the use of apparatus by actual trial and work, and not by precept only. The less talk and printing about the matter the better.

The following plan has proved successful in practical application. Let each man in the fire organization be assigned to a particular class of duties, and when an alarm sounds let him go to his appointed position and then await orders. A large printed card like the following with the names of the men written in the blank spaces, should be posted in each room.

FIRE ORGANIZATION OF MILL.

Chief, ...	The agent
First Assistant Foreman, ...	—— ——
Second "	—— ——
To stop and start engine (or wheels), ...	—— ——
Assistants {	—— ——
. . . .	—— ——
To put pumps in gear and stay by them, . . .	—— ——
Assistants {	—— ——
. . . .	—— ——
Foreman of Hose, ...	—— ——
Assistant Foreman of Hose, ...	—— ——
Leading Hosemen, ...	—— ——
	—— ——
	—— ——
	—— ——
	—— ——
	—— ——
	—— ——
	—— ——

The hosemen should be selected from men employed at different parts of the establishment. The overseers should not form a part of the organization, except to remain in their rooms, and there be subject to the orders of the chief.

Now for the mode of working. At regular times for trials of apparatus, as every second and fourth Saturday in each of the warm months, let the clerk of the Company summon a meeting of the men in the yard at an appointed hour. He may say, "There is a fire in the repair shop," or at any other designated place. If the water wheels are running, the wheel man, without any further orders, shuts down at once; the pump men put their pumps in gear, and the foreman of leading hose directs his men where and how much hose to connect, and as soon as he is ready the word is passed to the wheel man, and the pumps are started.

Then, after the original order is given, the chief leaves the men to do as their own judgment prompts them; and if they do it well, and get the water on the desired point without delay and without mistakes, it will make them feel confidence in themselves, and render them cool in case of actual fire, and if mistakes are made, they will remember them and avoid the like the next time.

Let this plan or a similar one be adopted at every meeting, and the men will get the habit of doing their work easily and rapidly, and if the chief be present when a fire occurs, he will not have to bawl and halloo to a crazy crowd, who work hard and do nothing; and if absent he may feel that all will be done well if any call comes. Of course the whole thing is this: in case of fire get the water on as quickly as possible and make no mistakes.

It is of the utmost importance that the watchmen, who may often constitute the whole force on the premises, should be

FIG. 96.

specially drilled in the use of the apparatus and instructed what to do in case of fire.

In smaller mills, where the number of repair hands or skilled mechanics is insufficient to form the whole of a fire company, some of the members must be selected from the operatives, and in such places it is frequently alleged that there is a difficulty in maintaining a fire organization in full numbers and efficiency, because the help are changing so frequently. In other matters no such excuse was ever offered to justify the stopping of any profitable machine or process; it is just as important to systematize the attendance upon the fire apparatus as upon any portion of the manufacturing plant which may be dependent upon the fire apparatus for its very self-preservation. In such a class of mills it has proved useful to furnish each member of the organization

with a metal badge, about the size of a silver dollar, with the name of the mills and the wearer's position in the mill fire organization engraved upon it. This is ordinarily worn on the vest of the possessor, and in case he leaves the employment or even goes temporarily away from the neighborhood of the mill, this pin is given to his successor, or to his substitute for the time being. This plan continually reminds one of his duty and secures a full organization always in the vicinity of the property.

Mr. Thomas J. Borden, of Fall River, devised a system of fire alarm which localizes a fire without the use of electricity. In each room is a bell-pull which strikes gongs in engine and boiler-room and repair shop; beside this is another pull which lowers a large card bearing the name of this room in these rooms. For day fires this enables the whole force of hosemen to be concentrated at the fire without delay. This mill is one large building with an ell; and, of course, this system is not applicable to an establishment consisting of numerous isolated buildings. A number of manufacturing corporations are provided with systems of electric fire alarms, with stations in each room, which ring bells at the homes of each member of their fire organizations, as well as in the mills.

WATER SUPPLY FOR FIRE PURPOSES.

It is a difficult task to determine the minimum limit of water supply for fire purposes; there is no maximum. In fact those fires which are put out by water are generally extinguished by small quantities, but such general results do not grant a release of one from making arrangements for the largest possible amount of water.

A mill fire system is probably the only case of water supply which any one will ever be called upon to arrange, without reference to any ultimate scheme for its present or future use for domestic or manufacturing purposes. Montreal is probably the only city in the world where there are two lines of water mains laid in the street—the one for fire and the other for domestic purposes.

At the great Boston fire, beginning November 9th, 1872, the amount of water used was enough to cover the whole burnt district to a depth of eleven inches, or the area occupied by buildings to the depth of thirteen inches.

The following are the quantities of water used at certain fires

in Fall River. The Holly system is used there, and the quantities charged for fire use is the excess of the consumption over average consumption.

		Gallons Used.	
	Hydrants.	At Fire.	Per Hour.
Granite Mills,	7	104,265	96,048
American Print Works,	25	487,995	160,356
Massasoit Steam Mill,	10	797,276	135,654
Marvel & Davol,	8	111,271	118,927
American Linen Co.,	22	948,754	201,292
Border City Mills,	11	1,444,600	182,400

But an opportunity rarely occurs for concentrating such large quantities of water, but such a large reserve is essential for use when the rapid application of smaller quantities of water has proved unavailing.

The fundamental axiom relating to mill water supply is not found in the works of Rankine or Weisbach, but Benjamin Franklin's aphorism, "Don't put all your eggs in one basket."

In different sources of supply, in different pumping apparatus, in different means of application, everything is in duplicate, so that an injury to a part will not disable the whole. There is no arbitrary standard of the quantity of water necessary for fire purposes, and instead of making any assumption, based on personal experience, I will give certain data as a basis for judgment, as an element in any specified case.

FIRE PAILS.

The most essential fire apparatus consists of pails of water. Their importance is shown by the fact that it is a matter of record that of the losses in mills *paid for* by the insurance companies, twice as many fires are put out by pails as by any other means.

This represents only a small proportion of the number of fires extinguished by pails of water, as there are numerous fires where the loss is so trivial that no claim is ever made, and they are known only by some casual remark of the agent months afterward. They do not form any portion of the record of losses, although of especial value in calculating the origin of fires, and the successful measures used in putting them out. It always causes a strong presumption on the part of the underwriters to have confidence in the administration of a mill where fires are energetically put out.

These pails must be kept full and used for no other purposes whatever. The best fire pails are made of strong, galvanized iron without covers, and they will last much longer if painted with hot coal tar or some of the asphaltum roofing compounds. This also helps to reserve the pails, as their black color makes them easily distinguished from the other pails used for washing or drinking water. It should be the duty of some individual to keep the pails full, examining them at least once a week, and replenishing the whole of the water before it becomes foul. A further reserve is furnished by casks of water kept in the porches or corners of rooms.

If there are public water-works, the system of the mill ought to be connected with the mains, but such water-works must not be the only means of water supply, because the water is frequently shut off, on account of repairs or alterations in the vicinity, and the street mains are frequently so small that any unusual draft or even the maximum daily consumption reduces the head to an inefficient pressure. City steamers attached to hydrants near a mill reduce the head so that there is no adequate supply at the mill.

Those mills that have their own reservoir without supplying other connections have one of the best resources, but, in addition, an efficient pump system is necessary, drawing its supply from a different source.

FIRE PUMPS.

Fire pumps differ from other classes of pumps according to their entirely different duty to perform. Paramount to all other conditions is the ability to withstand neglect and rough usage, start quickly, at all times throw large quantities of water, and maintain a constant pressure. They are of two classes,—direct steam pumps and rotary pumps.

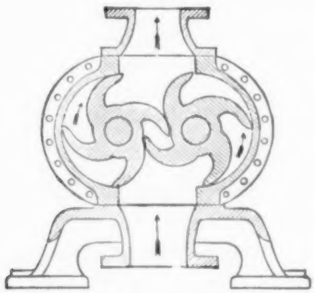
ROTARY FIRE PUMPS.

The rotary pump was originally invented in France, and consists of two coarse gears or their equivalent in a case, and their operation is due to the displacement of water already between the teeth on the extreme sides of these gears by the meshing of the teeth as the gears revolve. It is a coarse, mechanical movement. Consider the tortuous path of the water through the pump and

its rapid change of velocity due to the varying cross section. And yet for mill fire purposes, it is the best pump ever made.

There are no valves requiring attention or small parts to break; it wears out slowly, and can be repaired almost indefi-

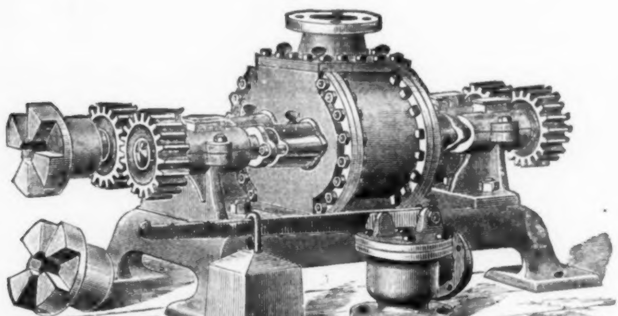
Fig. 97.



Section of a Holyoke Machine Company Rotary Pump.

nitely. A pump of given size and weight will move a greater mass of water than any other class of pumps. The first rotary (and, in fact, it is the first mill pump exclusively for fire protection) ever used in this country is in good working order at Allendale, R. I.

Fig. 98.



Holyoke Machine Company Rotary Pump.

A rotary cannot lift water successfully, therefore it should take its supply from the flume. I do not advise that it be placed so low that the water will run into it, but about two feet above the water level. When a pump is set below the water level—submerged as it is called—there must be a stop valve in the suc-

tion pipe; the water may percolate by this valve and freezing obstruct the pipe, and frequently break the pump, or some one will forget to open the valve in an emergency, and all concerned will declare that some inscrutable providence kept the pump from working.

Rotaries should never be driven by belts, and bevel gears are objectionable, because, with such arrangements, one of the gears generally slides upon the shaft and the thrust is excessive against the collars and arrangement of levers that throw it in gear, and, unless constant care be taken, the sliding gear will stick fast to the shaft. The usual method of driving them is by spur gear wheels, directly from the jack shaft, but the preferable method is by friction gears, which consist of wheels with wedge-shaped tongues and grooves turned upon their peripheries, which engage with each other.

When ordinary toothed gear is used, the wheels must be stopped, before engaging or disengaging the gears, or there is almost a certainty that they will be broken by the shock. With this apparatus the pump can be started without shock or jar with the wheel running at full speed.

FRICTION GEARING FOR DRIVING ROTARY FIRE PUMPS.

The following brief explanation will make the engraving understood: The framework, A, A, A, A, supports the pump, B, and the bearing for the driving shaft C, with its gear, in the usual manner; it also supports the plate, D, on which slides the plate E, carrying with it the short pump shaft and driving gear. The two gears are thrown into or out of mesh by means of the hand wheel and screw, D. The hand wheel may be placed in any convenient position for operating the screw, and connected with the screw D by shafts and bevel wheels.

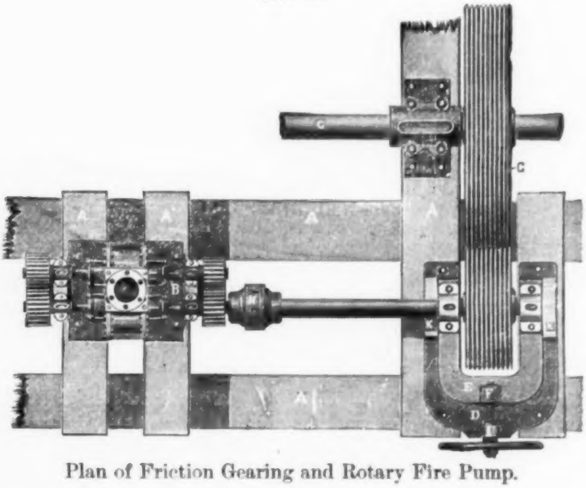
This method of transmission of power is referred to in Fairbairn's *Mills and Millwork*, vol. ii., p. 271, and Rankine's *Applied Mechanics*, pp. 431 and 618. When a pump is driven by friction gears, it can be put in operation without stopping the mill, and there is no danger of breakage by excited or incompetent persons, as frequently happens in the case of the use of spur gear.

When practicable, it is advisable to have the rotary pump driven by a separate water wheel, or, if driven by the main

wheels, have a clutch in the main shaft, so that the fire will not be spread by the currents of air caused by moving mechanism.

A rotary pump should have an ample check valve in the force pipe, to prevent the water in the pipes turning the pump backwards when the motive power is removed. Check valves are rarely of sufficient capacity. There should be a pet-cock in the top of the pump, always left open when the pump is not forcing water, to enable the pump to force out the air. Many pumps will not operate at all times, because they cannot force the air out by lifting the check valve. This is of the utmost importance in all except new rotaries, and it is to the lack of pet-cocks that rotaries are so uncertain in their action. Sometimes they seem

FIG. 99.



Plan of Friction Gearing and Rotary Fire Pump.

endowed with that "total depravity of inanimate things" which is a clause in the creed of many.

After trial, a rotary can be freed from water by turning it backwards, and then oiled with some heavy mineral oil, which can be rendered fluid enough by heating on an engine cylinder or over hot water, but never use lard oil, melted tallow, or any animal oil, as the acids which they contain will injure the pump. The principal rotary fire pumps are made by The Holyoke Machine Company, Holyoke, Mass.; Fales, Jenks & Sons, Pawtucket, R. I.; Holly, Lockport, N. Y.; Crocker, Turner's Falls, Mass.; Wiswell's Torrent, Boston; American Pump Company, Hartford, Conn.

It is unadvisable to use the rotary pumps for practice during freezing weather, but at such times the pumps should be moved by hand every week.

POWER PLUNGER PUMPS.

There are so few of them now in use as fire pumps that it is unnecessary to make any extended reference to that class of reciprocating pumps driven by power applied upon a pulley, and the circular changed to reciprocating motion by means of crank and connecting-rod.

These "power pumps" have been superseded by direct-acting steam pumps, so termed because the motive power of the steam meets the resistance of the water without any intervening train of mechanism.

STEAM FIRE PUMPS.

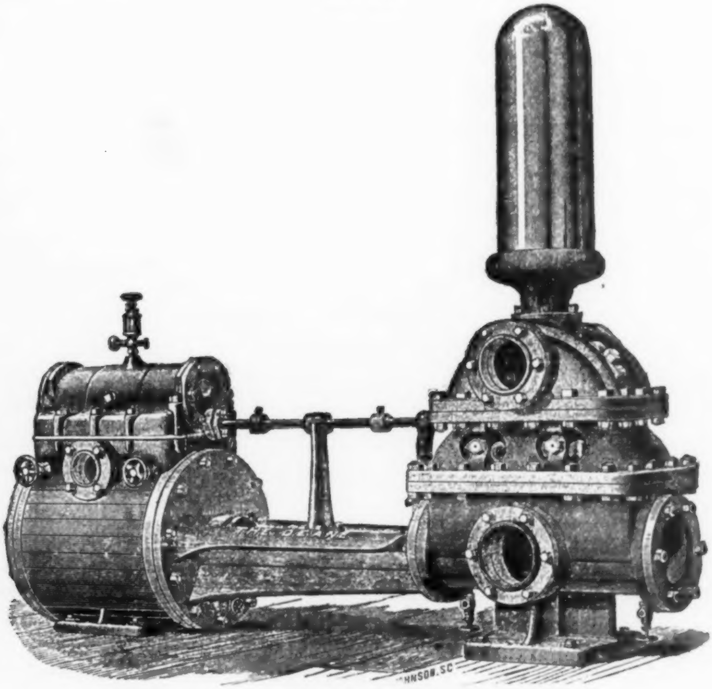
The problem in the construction of this class of pumps is the method of reversing the motion of the pump so as to secure its continuous operation; and their relative merits of design and operation are all primarily based upon the manner in which this is accomplished.

One method is by connecting a fly wheel so that its momentum will reverse the valve at the steam end. This system in fire pumps is open to other serious objections beside the introduction of heavy additional mechanism, involving expense, power, and risk. In moving water to the best advantage it is essential that its velocity be uniform, and, therefore, the plunger of the pump should move at as nearly constant velocity in all parts of the stroke as practicable. If a fly wheel is introduced into the system, its inertia resists changes of velocity, and it tends to revolve with a uniform rotary motion. The impulse of the steam, as used in these pumps, tends to move the piston with a uniform rectilinear velocity, except at the extremes of the stroke; the result of the motive force of the steam, constant resistance of the water, and inertia of the fly wheel, is that neither the circular motion of the fly wheel nor the reciprocating motion of the plunger is uniform. If the weight of the rim of the fly wheel is adequate to furnish sufficient momentum to reverse the pump at slow speeds, it is unnecessarily heavy for ordinary use. The energy stored in a revolving fly wheel is a constant threat upon the weakest part of the machinery, which is carried into effect

the instant the resistance of the water is suddenly increased beyond a certain limit, by any obstruction.

In another system of pumps, the stroke is reversed by means of a supplementary steam valve, which moves the main valve. This supplementary valve is opened by the agency of a small tappit, which is moved by the steam piston or its rod near the terminal of the stroke.

FIG. 100.



Deane Pump.

The principal pumps constructed upon this system are the Deane of Holyoke (shown in the engraving), the Blake, and the Knowles of Boston, and the Cameron of New York. Another method is the duplex system of pumps, invented by the late Henry R. Worthington, where two complete pumps and their steam cylinders are contained in the same frame, parallel with each other, and the steam valve of each pump is moved by the piston-rod of the other.

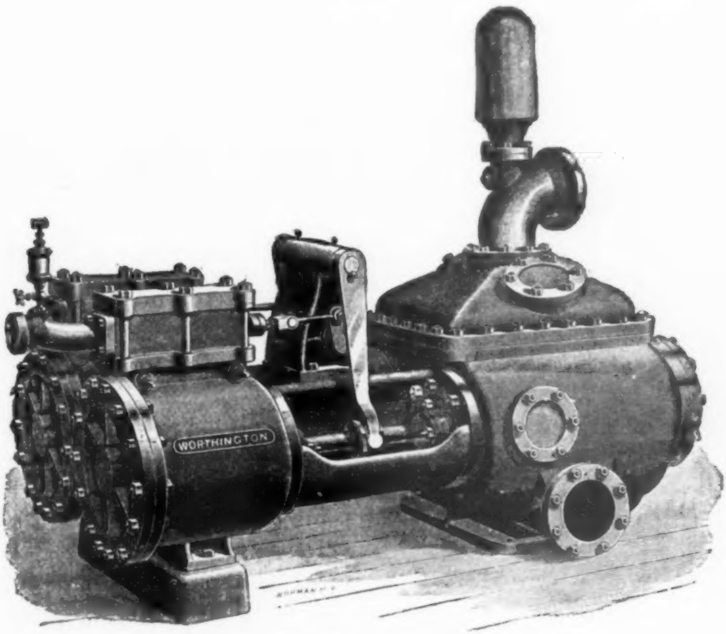
The direct-acting steam pump was invented by Henry R. Worthington, in 1844, and is the original of the whole class of direct-acting pumps.

The first steam pump especially for fire service was made by him, for the steamer "Bay State," in 1849, and operated satisfactorily for many years.

The diameter of the steam cylinder was twelve inches, the water cylinder six inches; the stroke was nine inches.

It had an arbitrary water valve motion, combined with steam valve, and moving simultaneously therewith. The steam valve was a balanced box slide valve.

FIG. 101



Worthington Pump.

Steam fire pumps differ from other pumps in the relative proportions which the steam cylinder bears to the pump,-the diameter being 2 to 1, so that the steam pressure is to that of the water as 1 to 4, minus the friction of the whole machine. This qualifies the pump for efficient work when the steam pressure is very low, as is apt to be the case during nights or Sundays. They are provided with large water-passages and swing-bolts or hand holes, so that the interior can be examined at short notice.

The steam fire pump should be set so as to draw its supply

from the wheel pit, that it may be independent of the supply in the mill pond, and be used to empty the wheel pit in case of repairs or renewals about the wheel. It is the general custom to use soft rubber valves in this class of pumps, but efficient sure action is here paramount to the quiet smooth operation of rubber valves. The refuse of lubricating oils, wool scouring, and dyeing collect in the tail race of a mill. Many of these substances, especially oils, affect such soft rubber valves, sticking them to their seats so firmly, that they can only be removed by cutting under with a thin knife. The ordinary substitute for soft rubber pump valves is brass, but brass valves are soon cut if grit is suspended in the water, and then leaking begins. Where oils, dye stuffs, or chemicals are discharged into the stream, and there is any liability that they will be drawn into the pumps, Jenkins's pump valve should be used; I have tried its endurance by immersing pieces of it for several months in the oils and corrosive chemicals, used in cotton and wool manufacturing and dyeing, and it was not in the least affected by any of these substances; but for ordinary service, with pure water, soft rubber is satisfactory, as it forms a tight and noiseless valve. If a steam fire pump is let alone, it will be disabled by rust, and, to insure its frequent use without wearing it out, it is advisable to connect it with the tank that supplies the sink and water closets, so that it will be necessary to use it for a few minutes every day. Just before stopping, the cylinder of a steam pump should be lubricated with mineral oil, never using sperm or lard oils, or tallow. The pump should be stopped at the middle of the stroke, and the drip valves opened. The principal steam fire pumps are made by H. R. Worthington & Co., New York; Deane Steam Pump Company, Holyoke; Blake and Knowles, Boston; Cameron, New York. Pumps are frequently disabled by foreign matter in the suction pipes, which should be protected by a strainer, the aggregate area of whose orifices should amount to five times the area of the cross section of the suction pipe; and, in many places, it is desirable to guard this strainer in the direction of the current by some barrier which will prevent the deposition of leaves and rags upon the strainer. If the suction pipe is long, there should be a foot valve at the bottom, and especial care is necessary to arrange one of ample area of opening, so as not to throttle the water supply to the pump.

RELIEF VALVES.

The ordinary lever safety valve cannot open far enough to discharge a sufficient quantity of water to reduce the pressure, and, when in working order, cannot be considered more than an alarm, indicating an increase of pressure which it is powerless to remedy. Iron safety valve seats rust down very frequently.

A relief valve of ample capacity should be connected to the force main near the pump: it will save the hose and pipes from injuries caused by sudden or excessive pressure.

Relief valves are constructed on the general principle of that class of safety valves operating by the reaction of the fluid, and present a larger area of opening than any other class of safety valves. The best relief valves have non-corrosive seats made of nickel alloy, and the spiral spring holds the valve to its seat by tension instead of compression, and, therefore, the action of the valve cannot be reduced by reason of the chips, gravel, and pebbles, liable to collect in a valve of this form. Such a mishap could never happen to a well-kept steam fire engine of a city department, but it is of frequent occurrence in a country factory village, where the small boys run loose, and can give attention to such matters.

WATER-PRESSURE GAUGES.

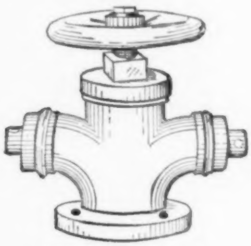
It is very desirable that a pressure gauge be connected with the force main, and placed in a position where it can be clearly seen by the man operating the pump, enabling him to maintain the desired pressure, and also to control matters in case of a break anywhere. Such a gauge should not have any siphon in the tube connecting it with the force main, but the tube should be descending so that no water will remain therein after the pressure is removed. If there were a siphon or bend in the tube which would hold water after the pressure was removed, there would be great risk of damage by freezing.

HYDRANTS.

The hydrant in general use is the common Y or branch hydrant, and is not provided with any means of draining off the water when the hydrant is closed; and I suggest the advisability of boring a hole about $\frac{3}{32}$ of an inch in diameter, through the shell of the hydrant just above the seat.

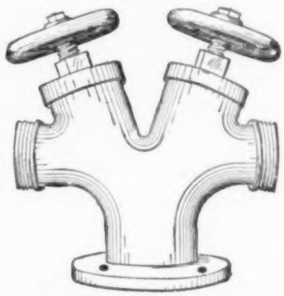
When the pressure is on the pipes, water remains in the upper portion of such hydrants as were closed before the pipes were drained, and also forces its way into the upper portion of every hydrant that does not remain perfectly tight under the heavy pressure. The hydrants are generally tight enough to retain this entrapped water, and the hydrant caps prevent its evaporation. A large amount of fire apparatus around mills is broken by ice every year, and this damage could be prevented by ordinary foresight. The hazard due to a single broken hydrant is not limited to the possible deprivation of its use, but is chiefly due to the fact that when water is forced into the pipes there is great risk of the hydrant breaking; and in most mill yards such an accident would tap all the pipes and prevent the efficient operation of the fire apparatus. The pos-

FIG. 102.



Double Hydrant.

FIG. 103.



Double Valve Hydrant.

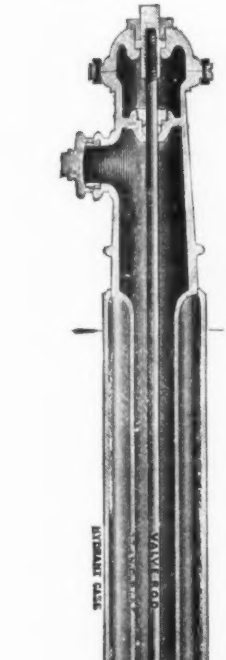
sibility of such accidents can be obviated by opening all the hydrants when draining the pipes for the winter months, and closing them afterwards. The drip of the system should be in plain sight; extending out over the tail race is a very good place in most instances.

All valves, gates, and connections should be easily accessible, and not placed in dark corners, closets, or spaces under trap-doors. When the purpose of the valve is not absolutely evident from its position and surroundings it should be labelled.

The hydrants in general public use are called post hydrants, but their cost retards their adoption in many places where really needed, in the yards of manufacturing corporations.

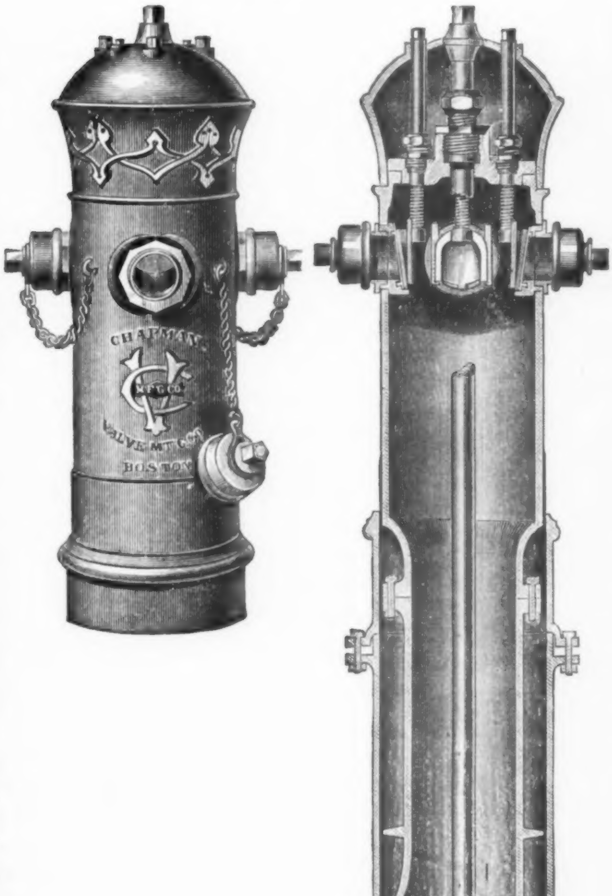
The Mathew's hydrant, made by R. D. Wood & Co., of Philadelphia, has been as widely introduced as any post hydrant, and has given general satisfaction.

Fig. 104.



Mathew's Hydrant.

Fig. 105.



The Chapman Fire Hydrant with Independent Valves for each Hose Nozzle.

The valve is below frost at the level of the distributing main, and danger from freezing is obviated by an automatic drip. The capacity of these hydrants is sufficient to supply several lines of hose.

The Chapman Valve Company of Boston has recently introduced a new post hydrant, which in addition to the main valve at the bottom, has independent valves at each of the four outlets, enabling one person at the hydrant to control each of the four streams as absolutely as if the streams were taken from different hydrants with a man at each. The gates in this hydrant are metal wedges, with Babbitt seats, similar to the principle involved in the design of the well-known Chapman valve, to which further reference will be made.

Yard hydrants should not be placed much nearer buildings than the height of the walls, so that a falling wall will not injure the hydrant. Hydrants should be oiled with heavy mineral oil, as stated in the case of rotary pumps, and never with tallow or lard oil. The most efficient cheap protection from freezing, I believe to be a barrel filled with burrs from the picker of a woollen mill. Wool waste of any kind forms a good protection. The covers of gate pits should have handles high enough to be always visible above snow, before the paths are made.

STAND PIPES.

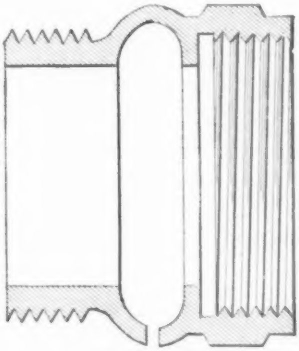
Stand pipes on the fire escapes of mills are not now in vogue, as experience has shown that any fire hot enough to drive men out of the room, cannot be fought from the galleries in front of the windows. Where there are several buildings, Mr. Edward Atkinson's plan of pipes running to the roof, and then fitted with a system of hydrants, is very efficient. The base of these pipes should be connected with the mains by a gate, or to the yard hydrants by hose coupled to the lower end of the standpipe, so that the whole system will not be disabled by the leakage from a broken pipe, if the wall falls.

A vertical pipe should extend to the height of the porch tower, with hydrants at each story and on the roof. To each porch hydrant a length of hose should be connected and provided with a drip coupling.

The writer believes that the drip coupling was first designed and made about nineteen years ago by James G. Brackett, Su-

perintendent of the Laconia Mills, Biddeford, Maine. In its present form, it is now manufactured by the Providence Steam and Gas Pipe Company. It consists of a coupling with a sudden enlargement which serves as a pocket to collect any leakage

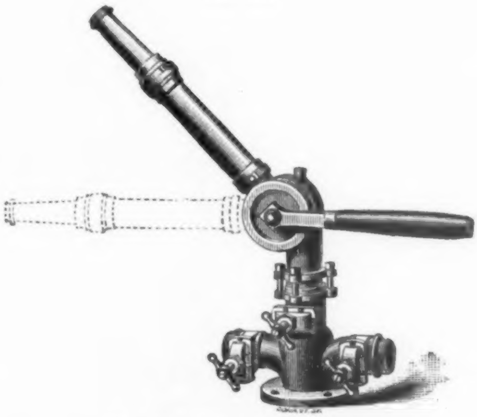
FIG. 106.



Drip Coupling.

from the hydrant, from which it flows through a small orifice instead of wetting and destroying the hose. The hose should be doubled and wound upon a reel, beginning with the middle,

FIG. 107.



The Morse Monitor Nozzle.

so that one can take hold of the nozzle and run, without being impeded by a snarling hose.

There are several forms of adjustable spray nozzles, which can

be changed from a solid stream to a fine spray by the person at the nozzle. These are well adapted for inside work.

The Monitor nozzle furnishes a valuable addition to mill equipments, especially on roofs in positions commanding lower buildings. The recoil of the water being perfectly balanced, the nozzle can be pointed in any direction, and will remain in that position independent of the force of the stream.

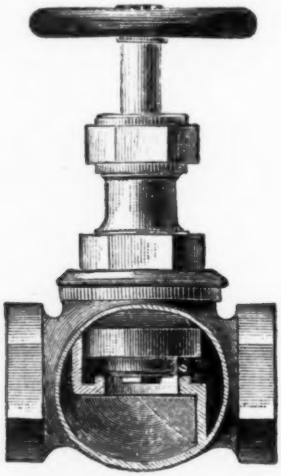
After a system of water pipes and hydrants has been laid, it should be very thoroughly washed to remove the last traces of the sulphuric acid,—"pickle,"—used in cleaning the castings.

This is often very imperfectly washed off, in the haste of manufacturing, and the writer has seen many instances where the new hose attached to porch hydrants in mills, has been destroyed by this cause, where the leakage was so slight as to be imperceptible.

SMALL HOSE.

Small hose, connected with a constant head furnished by a tank or reservoir are very useful, especially in carding-rooms. It is desirable that there should be no shut-off in the nozzle. The valve where the small hose is connected to the feed-pipe answers every purpose, and a second one in the nozzle is needless; and such small shut-offs are very often stuck by corrosion, so that they cannot be opened by the fingers.

FIG. 138.



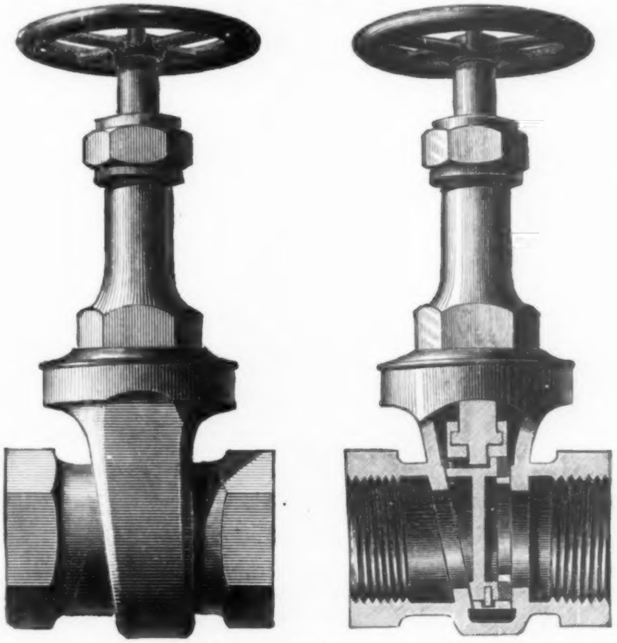
Globe-Valve.

VALVES.

Only straightway valves should be used for water.

In some experiments which the writer made at Holyoke, a two-inch globe-valve reduced the water pressure from 80 to 40 pounds per square inch, while a straightway valve substituted, and operating under identical conditions of supply, reduced the pressure from 80 to 71 pounds per square inch. This sectional view of a globe-valve shows its irregular winding passage, giving a great frictional resistance to the delivery of water.

FIG. 109.

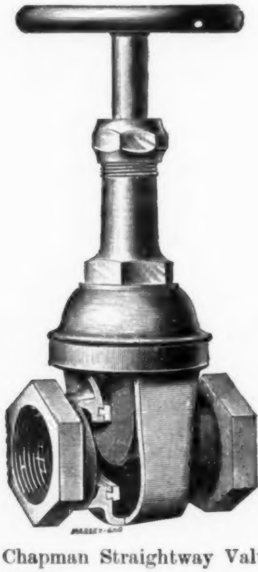


Jenkins Straightway Valves.

Valves are often broken or damaged by being closed too hard. A valve or hydrant which is not water-tight when closed by hand without a forcible effort is surely not in good order, and is liable to cause damage by the breaking of other strained parts. Never use a valve which is without an advancing-stem; it is always perplexing not to know the position of the valve, whether shut or how far open, and the disasters charged to inoperative valves are generally traceable to this cause or to left-hand valves.

If beyond your power to replace the left-hand valves by right-hand valves, label them by an arrow, and the word "open," painted on a piece of tin fastened to the spokes of the hand-wheel of the valves. A right-hand valve shuts in the direction of the motion of the hands of a watch, and a left-hand valve in a contrary direction. This is the common and general use of the term, and is so used by the writer, but in some localities these expressions are used in exactly the contrary manner; and, therefore, where there is any chance to be a doubt in the matter,

FIG. 110.



Chapman Straightway Valve.

it is best to specify that it is the direction in which the valve shuts. For straightway valves the Jenkins and the Chapman furnish good examples.

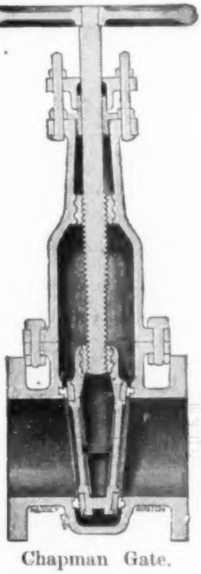
The Jenkins valve presents a full opening, the advancing-stem shows the position of the gate, and it is a right-hand valve with advancing-stem. It is provided with Jenkins packing, which can be renewed, when necessary, in a moment, by the substitution of a new ring of the packing.

The Chapman valve has a gate of composition in the form of a wedge, which presses against two Babbitt metal seats. Several styles of valves are made by this Company, but the writer now refers in commendation only to their right-hand valves

with advancing-stem. These valves are more generally used than any other about sprinklers.

This Company have recently made a right-hand gate with advancing-stem provided with compound screws, so that it can be opened or shut by one turn of the hand-wheel to every inch in diameter of the gate; that is, a five-inch gate is shut or opened by five turns of the hand-wheel. In addition to the merits of the Chapman valve-seat, this gate has the advantage of opening, in the natural manner; advancing-stem, full opening, and the utmost quickness of motion without water-hammer.

FIG. 111.



Chapman Gate.

HOSE.

Only a small portion of the hose in a mill is subjected to wear, and frequent use, being kept in reserve; a few lengths are used for practice of fire organization; therefore, the most important specification demands some durable material, which will not deteriorate by age, and is always ready for use. Experience has proven unlined linen hose to be the best for inside hydrant and reserve use. It is light, flexible, and strong; twelve samples from different manufacturers weighing from $3\frac{3}{4}$ to 4 ounces per foot, and bursting when new at pressures of 420 to 650 pounds per square inch.

If kept dry, it will last for an indefinite time. It can be fairly protected from mildew by treating it by a solution of paraffine wax dissolved in naphtha.

For outside use, rubber lined cotton hose fulfils the demand for strength and durability, bursting at pressures of 800 to over 1100 pounds per square inch when new, and weighing 12 to 20 ounces per foot.

However suitable rubber-lined linen, rubber and leather hose may be for public fire departments, both experience of use and tests show that they are not well adapted for mill use as the kinds mentioned above.

It is essential that all hose couplings at an establishment be uniform, and desirable that they be like those of neighboring factories and the public fire department. If there is an unavoidable difference there should be a supply of reducing couplings in accessible places.

Except the work by the Providence City engineer's department, there has been very little investigation of the flow of water through valves, hose and nozzles. The reader is referred to an admirable work on this subject, "Power Required for Fire Streams," etc., by George A. Ellis, C.E., of Springfield, Mass.

As a basis for computations estimating the diameter of distributing mains supplying the hydrants of a fire system, the following table for the discharge of water through one hundred feet of rubber hose and a one-inch smooth nozzle is taken from the results given in the work cited above:

Pressure at Hydrant. Pounds per square inch.	Discharge per minute. Gallons.	Distance reached by jet. Horizontal. Feet.	Vertical. Feet.
15	84	54	26
20	98	62	35
25	112	72	45
30	122	80	52
35	132	88	60
40	140	96	67
45	149	103	75
50	157	111	80
55	165	118	88
60	172	125	93
65	180	132	101
70	186	139	106
75	193	145	111
80	199	150	116
85	205	156	121

FLOOD PIPES.

The protection of dry and picker rooms, paint shops and other dangerous parts of manufacturing property, is often supplemented by placing such rooms below the level of the mill pond, and running a large pipe with a gate, from the forebay to each of these rooms. Some old mills contain pipes which lead from the pump to the attic floor, but these crude forms of apparatus have been superseded by other methods, which concentrate water at any place in a mill.

SPRINKLERS.

For many years, the more hazardous portions of mills have been defended against fire by parallel lines of perforated pipes, extending across the room near the ceiling, and connected with a water supply furnished by pumps, reservoir, or tanks, so that the room may be showered by opening a valve on the outside of the building. This arrangement is particularly valuable in rooms difficult of access from the outside, where the contents are very combustible, or where the smoke is too pungent for human beings, as that of loose cotton.

There are several systems of sprinklers. Mr. James B. Francis at Lowell places the perforated pipes against the ceiling, running across the mill in the middle of each bay. The orifices are one-tenth of an inch diameter, and placed nine inches apart, alternating on opposite sides and bored a little above the horizontal diameter of the pipe. The supply pipes have twice the sectional area of the orifices, and the distributing pipes are also of double sectional area to the orifices which they supply. With a head of forty feet the discharge of the system is one-fourteenth of an inch deep per minute, or four and two seventh inches per hour. The reader is referred to an article on this subject by Mr. Francis, in *Journal of Franklin Institute*, April, 1865.

Mr. William B. Whiting has modified the Francis system, with a view to accomplishing a more general distribution of water, and using less pipe, by running the perforated pipes longitudinally with the mills under the beams, and drilling orifices three inches apart, and one-twelfth of an inch diameter, alternating on the top of the pipe, and thirty degrees from the vertical on each side, or in millwright's phrase, at ten, twelve, and two o'clock.

The Providence Steam and Gas Pipe Company use substan-

tially the Whiting system, but bore the orifices one-fourteenth of an inch diameter and four inches apart. The discharge of this system under a head of forty feet is .033 cubic feet to each orifice per minute, or nearly one quart. When the pipes are placed fifteen feet apart, the discharge from the whole system amounts to one cubic foot per minute, to every one hundred and fifty square feet of floor, covering the floor to a depth of .08 inch. The sectional area of feed-pipes in all of those systems should be twice that of the discharging area of the orifices when the supply proceeds from a reservoir, but if the supply is furnished by pumps capable of sustaining a high pressure, the area of feed-pipe sections may be one and one-half that of orifices, or even less, if the perforated pipes are short, and the water pressure large.

To furnish an adequate water supply to a system of sprinklers, the quantity must not merely be ample to cause a stream to flow from each orifice, but also with a force to impinge strongly against the ceiling above. In experiments made upon sprinklers at Holyoke, the writer found that in some cases the various frictional elements reduced the head from 150 feet to 2 feet. None of the books on Hydraulics give coefficients of friction and efflux for the solution of these problems, and the only method is to follow those precedents which are proven satisfactory in practice.

The efficiency of sprinklers is liable to be impaired by rust and paint obstructing the orifices. When the pipes are being painted, much time and trouble can be saved by placing tacks in each hole, and removing them when the paint is dry. The matter of rust is anticipated by boring larger holes than necessary, and then driving in a brass bushing. The superintendent of the mill of the Agawam Canal Company at West Springfield, Mass., first used such brass bushings about twenty-two years ago, and he also set up the sprinklers in the mill yard, and experimented with orifices of various diameters until satisfactory results were reached.

The corrosive vapors in paper mills and bleacheries cause all iron orifices to be choked by rust. Mr. William B. Whiting anticipated this by using brass rosettes, covered by a loosely fitting cap placed about ten feet apart, and connected by a system of pipes without orifices. The valves at the distributer supplying the sprinklers should be protected from meddlers by a

glass covering, but accessible under all conditions of affairs. A hole not over one-eighth of an inch diameter should be bored in the casing of the valve just above the gate of the valve.

Pet-cocks are often placed in the coupling just above the valve; this is too high, as it permits enough of the water that percolates by the shut valve to remain there and break the valves by freezing; it should be in the shell of the valve. There should be a half-inch drip valve on the distributer kept open, and another beneath the gate. In some mills it is the duty of the watchman on entering for his night's work, to open this lower drip and make a memorandum that the water was all right to the sprinklers. These records are carefully filed away. The sprinkler valves should be clearly labelled. Notwithstanding the absolute necessity of such apparatus, the use of this system has been open to serious objections. Property is sometimes damaged by water let on the sprinklers by accident or malice; the orifices in the sprinkler pipes are liable to be obstructed by paint or rust; and in case of emergency, the water cannot be concentrated on the fire, but will be spread over the whole or half of the room covered by that system of sprinklers. The efficiency of the best planned system of pipe sprinklers is limited by the vigilance of the one discovering the fire, and the presence of mind which *opens the right valve*.

AUTOMATIC SPRINKLER.

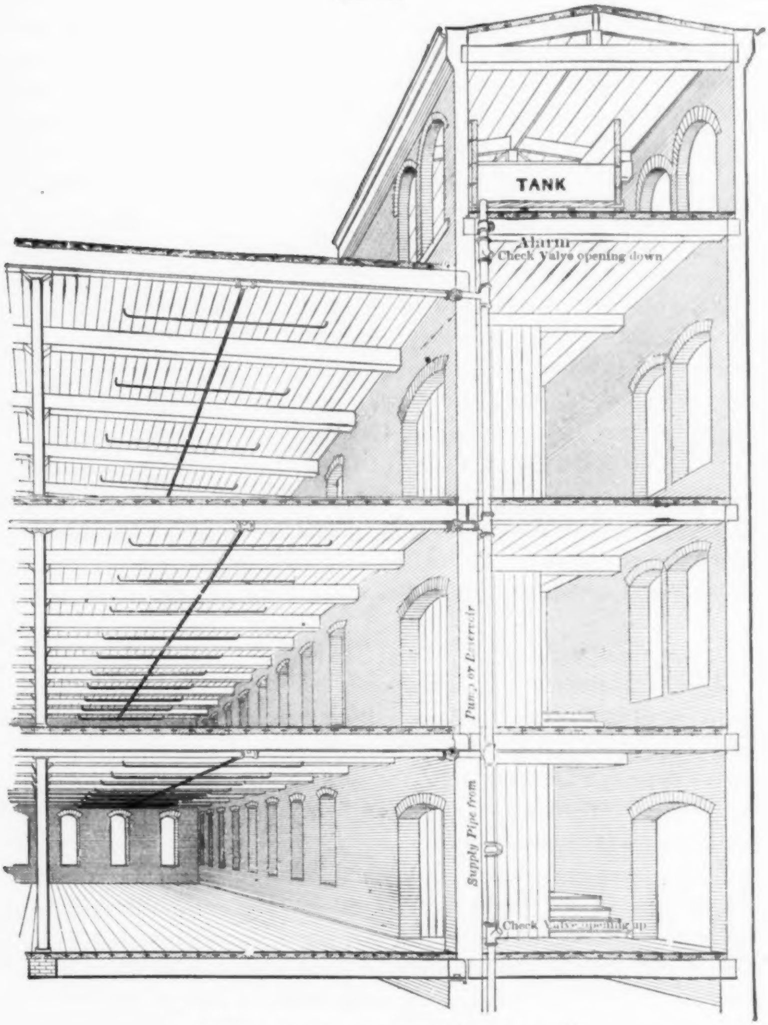
Although continually reinvented, this is one of the oldest devices for special fire apparatus. In 1806, a patent was granted in England for a "shower bath for checking fires in apartments and warehouses." The valve in the supply pipe was opened by a weighted lever, which was secured by a cord which passed over different parts of the room.

There have been many inventions wherein eternal vigilance was alleged to be obtained by cords burning and releasing the valve which shut off the water, but experience proved that the cord was first to break in peace and the last to consume at fire; the valve sticking to its seat like a leech saved many an establishment from flooding whenever accident broke the cord.

Within a few years there has been another type of sprinklers, termed from the method of their operation "Automatic." They each consist of a rose-head, or its equivalent, for throwing water in an upward direction, with even distribution, over as large an

area as practicable. The distributing pipes and their branches are arranged so that these heads are about one foot from the ceil-

Fig. 112.

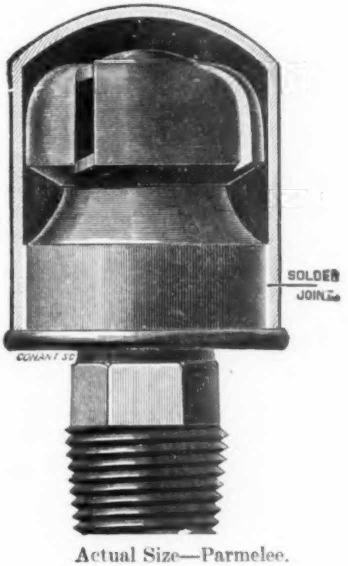


System of Automatic Sprinklers in a Mill.

ing and ten feet from each other. These heads are kept closed by thin metal caps over the top or valves within, either being secured by bismuth solder, composed of one part each of tin, lead and cad-

minum, and four parts of bismuth. This bismuth solder melts at 165° Fahr., but loses its tenacity at 155° Fahr.; for dry rooms and similar places alloys which fuse at 212 or 250 degrees are used. The first fire ever extinguished by means of fusing metal was probably at the Cathedral Building in Boston in 1870, where a fire, caused by spontaneous combustion, gained considerable headway, and melting a lead water pipe, was extinguished by the water which flowed from the open pipe. The accompanying diagram of a part section of a mill shows the general arrangement of automatic sprinklers. The water pressure remains constantly

FIG. 113.



Actual Size—Parmelee.

upon these sprinklers, and, when the heat in any portion of the building exceeds the melting point of the solder, the head is opened and the water flows from the nearest sprinklers. The first supply is usually from a tank placed in the mill tower, and this is supplemented by a reserve supply furnished by pump or reservoir. The lowering of the water in the tank sounds an alarm, by means of an apparatus which rings a bell or blows a whistle.

In some establishments, this alarm is used to blow the whistle at morning, noon, and night, by opening a faucet and drawing a little water from the tank, thus serving the double purpose of

marking the hours of labor and giving frequent assurance of the condition of the protective apparatus.

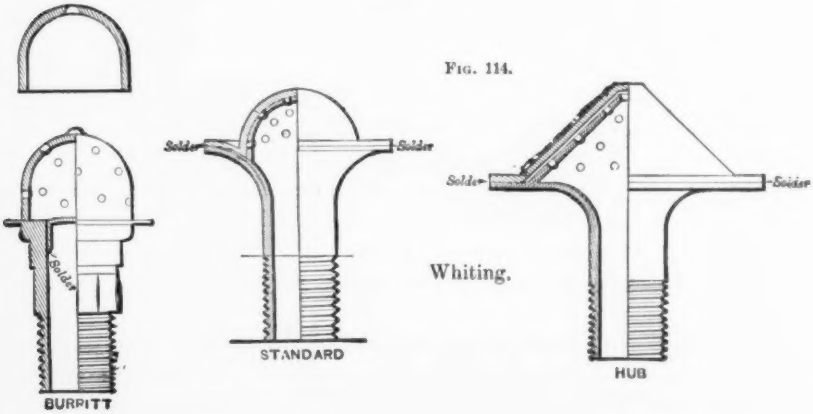
Of the automatic sprinklers in general use, the first was invented by H. S. Parmelee, and consists of a reaction turbine, covered with a cap soldered near its lower end. These sprinklers have been extensively introduced by the Providence Steam and Gas Pipe Company.

Some experiments on the discharge of these revolving heads gave—

Pressure.		Discharge.		
2½	pounds.	.688	cubic feet	per minute.
5	"	1.040	"	"
10	"	1.488	"	"
15	"	1.840	"	"
20	"	2.127	"	"
25	"	2.416	"	"
75	"	3.936	"	"

This system of automatic sprinklers is no new and untried experiment, as its representatives have constructed buildings in which hundreds of fires have been set and successfully extinguished. There have been thirty fires in mills put out by automatic sprinklers, and in no case, where properly supplied by water, have they proved inadequate to serve their purpose. The Standard and the Hub Sprinklers, invented by Mr. Francis W. Whiting, consist of rosettes with the cap soldered on at an annular flange. In this particular they differ from the other sprinklers, as the strain comes upon the bismuth solder at right angles to the soldered surfaces. In all other sprinklers

FIG. 114.

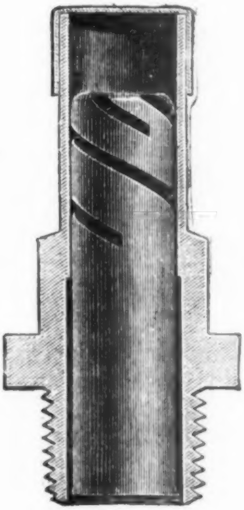


Whiting.

this is a shearing strain. The Hub differs slightly in shape, and quicker action is obtained by a sheet of felt, which serves as a non-conductor between the cap and the head.

The Burritt Sprinkler also consists of a rosette with a thimble soldered in the neck below the head. When the thimble is re-

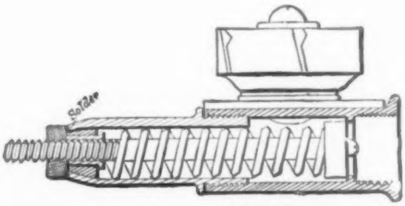
FIG. 115.



Bishop.

leased by heat, the water causes it to whirl rapidly around the inside of the head for the purpose of dislodging scale and sediment, which would otherwise clog the orifices. The cap is merely to keep out the dust.

FIG. 116.

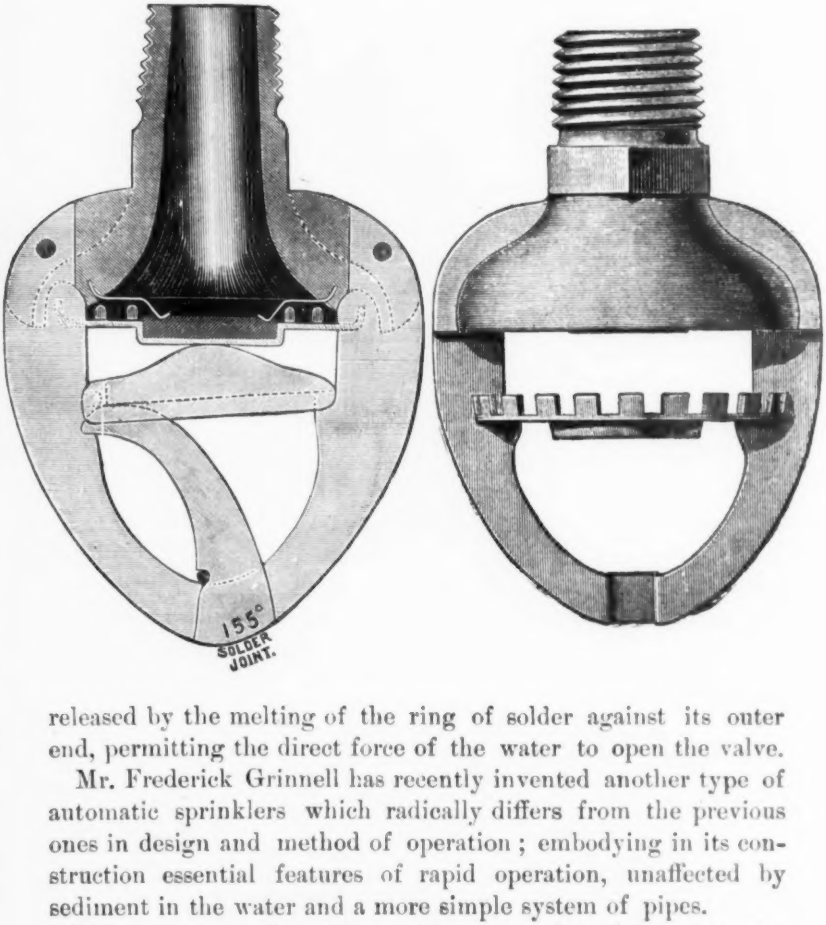


GRANGER

The Bishop consists of a tube with a cap soldered at the top. When the solder melts, the inner sleeve rises and distributes the water through the helical slots shown in the engraving. All

the previous sprinklers have sealed heads. Mr. A. M. Granger has invented a sprinkler which consists of a reaction turbine, from which the water is shut out by a valve packed with vulcanized fibre; the valve is kept closed by a spring, which is

FIG. 117.



released by the melting of the ring of solder against its outer end, permitting the direct force of the water to open the valve.

Mr. Frederick Grinnell has recently invented another type of automatic sprinklers which radically differs from the previous ones in design and method of operation; embodying in its construction essential features of rapid operation, unaffected by sediment in the water and a more simple system of pipes.

By reference to Fig 117, it will be seen that the opening for the escape of the water is at the centre of a flexible metal diaphragm; this opening is surrounded by a ring forming a valve-seat. The valve consists of a disk of soft non-corrosive alloy, fixed upon a circular metal plate with a crowned notched edge, which also serves as a deflector for distributing the water. The

valve is held to its seat by a pair of levers which are fastened together at their lower ends by bismuth solder. When the solder is melted the levers fly apart, releasing the valve, and the stream of water impinges upon the plate, and by the peculiar form of the edges is thrown upward and horizontally in a fine evenly distributed spray, wetting ceiling and floor.

This form of construction relieves the solder from excessive strain, and the solder is much more sensitive to heat than it is in any sprinkler where the solder is in contact with the large mass of metal forming the body of a sprinkler containing water, for the reason that under such circumstances the solder cannot melt until the water and surrounding metal is raised to the melting temperature of the solder.

This valve construction embodies a very peculiar result, inasmuch as the water pressure holds the valve securely to its seat, until the application of heat, and then the water pressure opens the valve.

The area of the annular diaphragm being three times that of the opening, the pressure of the valve against its seat is two-thirds that of the whole force of the water upon the diaphragm; and therefore when the head of the water varies, or there is a "water hammer," the valve instantly conforms to the condition of affairs and prevents leakage.

When the resistance of the levers is removed by the solder melting, the water pressure opens this valve.

As the orifice is about one-half of an inch in diameter, there is no liability of obstruction by foreign matter in the pipes.

This sprinkler differs from others by hanging below the pipes to which they are attached, therefore the pipes are not arranged below the beams in the manner shown in Fig. 112, but they are fastened close to the ceiling, crosswise of the mill in the centre of each bay between the beams, above the belts and shafting; similar to the arrangement of perforated sprinkler pipes adopted by Mr. James B. Francis, to which previous reference has been made.

The portions of a cotton mill where protection by sprinklers is essential are picker rooms, including dust rooms, card and mule rooms, the flue from the shearer, dry rooms, attics, and all concealed spaces. In special cases, other parts of an establishment require protection.

All of these sprinklers have been tested by experimental fires

in buildings especially constructed for the purpose; but none of them, except the Parmelee, have been subjected to the test of an accidental fire. It is the experience of the Mutual Companies that a cotton mill building has annually only one-fortieth of a chance of burning enough to call upon the underwriters for indemnification; small fires are more frequent, and the chance of total destruction by fire is only about one in three hundred. For details of these experimental fires, reference is made to the report of the New England Cotton Manufacturers' Association, April 24, 1878; *American Machinist*, New York, July 9, 1881; *Boston Journal of Commerce*, June 11 and July 9, 1881.

In some experiments made by the Insurance Companies, these automatic sprinklers all performed their work efficiently, and the preference between the various kinds must be decided by questions of mechanical construction of the several heads and the assurance that the contracting parties will put in a pipe service wisely designed to adequately conduct the maximum quantity of water which may be required of it, and that the workmanship will be so thorough that there will be no damage to the property caused by leaking or breaking of the apparatus.

In one of the fires a row of barrels were placed under each row of sprinkler heads, and upon laths placed thereon fifty pounds of cotton batting were spread. The windows and doors were closed, and the cotton lighted simultaneously in six places. The whole mass was instantly in a blaze, but the fire was smothered by lack of air. There was a different sprinkler head on each of the six openings, but the heat was so slight and of so short duration that none of the sprinklers opened. It was considered that this trial was of especial value, in showing the importance of closing a room tightly during a fire.

XLIV.

COFFIN'S AVERAGING INSTRUMENT.

BY JOHN E. SWEET, SYRACUSE, N. Y.

This opportunity of presenting a new invention gives me much pleasure. The invention, I fancy, must be of scientific interest to the technical members and of practical interest to all who have anything to do with the steam-engine indicator. It is a little

instrument or device of the nature of a polar planimeter, but one that goes beyond, and gives, without any mental or mathematical

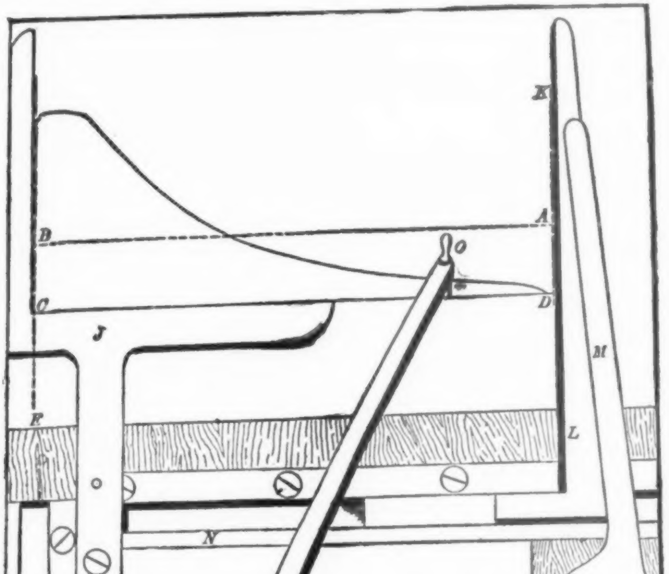


Fig. 118.
Coffin's Averaging Instrument.

calculation whatever, the average width of the card. It determines two points whose distance apart, if measured with the

scale which corresponds with the indicator spring, shows at a glance the mean effective pressure of the steam.

The instrument, as shown at *H* in Fig. 118, is a small affair, almost the essence of simplicity, and is arranged to go in the till of a steam-engine indicator-box. It is the invention of John Coffin. To use the instrument, it is only necessary to place the indicator-card, *P*, under the clamps, *J* and *L*, provided, and adjust one end of the card to exactly correspond with the edge of the stationary clamp, *J;* then move up the sliding clamp, or T-square, *L*, guided by the groove, *N*, so that its edge will correspond with the other end of the card. This clamp, when once adjusted, is held by the

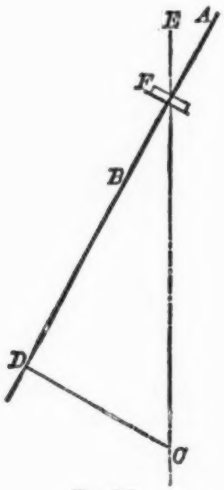


FIG. 119.

spring, *M*. The tracing-point, *O*, should be placed at *D*, and the pivoted-block, at the end, *Q*, placed in the groove, *I*. Set the wheel, *G*, at zero, and start with the tracing-point, where the card touches the T-square at *D*. Trace around the card with the tracer until it arrives at the starting-point, and make a slight indentation in the paper at *D*. So far the action and the result have been the same as the polar planimeter, and the area of the card can be read off from the wheel in the same way. But continuing the operation by carrying the tracing-point, *O*, up along the T-square *L*, until the wheel again returns to zero and making another indentation, the distance between these two indentations will be the average width of the card.

The action of the wheel, *G*, in this instrument is, in principle, like the polar planimeter. Referring to Fig. 119, let *A B* be the axis, and *F* the wheel; then, if the axis is kept parallel with its present position, and the device moved down the line, *E C*, the wheel will be rotated through a space equal to *D C* on its circumference, for the result is the same as if it were slid down the line *A D* and then rolled on the line *D C* to *C*. This is apparent if one imagines the wheel to have teeth, and the paper (shown at *E*, in Fig. 1), also to have a corresponding set of teeth parallel to the axis. Now take a figure like the one shown in Fig. 120. All the motion the wheel gets in traversing the horizontal lines will be neutralized by a corresponding motion in the opposite direction. In traversing the vertical lines the wheel indicates an addition or subtraction. For instance, in traversing the rectangles represented by dotted lines extending out from the line *B F*, going in one di-

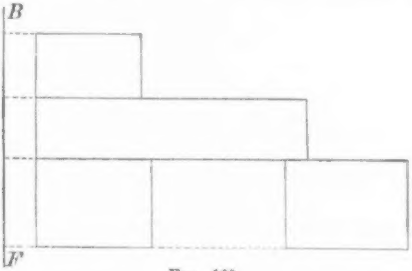


Fig. 120.

rection indicates addition, while going in the opposite direction indicates subtraction, so that it is plain the result must be the area of the figure. Any figure may be broken up into a number of steps, as shown in Fig. 120, and measured correctly. As the correctness of the machine in measuring areas has been proven, its averaging function may now be subjected to a like test. Taking, for example, the irregular-shaped card, *P*, shown in Fig. 118, the card touches the parallel vertical lines, *B F* and *D K*. We have started at *D*, traced around the card, and find the area to be $3\frac{1}{2}$. With the tracing-point at *D*, we set the wheel at zero, and trace up the line *D K* until the wheel reads "$3\frac{1}{2}$," or an amount corresponding to the area of the irregular figure represented by the card. We will suppose the point at which this occurs to be at *A*. Then the rectangle *A B C D* has the same area as the irregular figure or card, and the side *A D* is equal to the average height of the figure representing the mean effective pressure upon a card, and may be

measured by the scale corresponding with the spring used in taking the card. The same result would be attained by starting at D and going around the figure to the right, and then up the line, $D\ K$, until the wheel indicates zero.

XLV.

NOTE RELATING TO THE PROPER METHOD OF EXPANSION OF STEAM AND REGULATION OF THE ENGINE.

BY R. H. THURSTON, PROF. MECH. ENG., STEVENS INST. TECH., HOBOKEN, N. J.

It has long been well known, to every engineer experienced in the construction and management of the steam-engine, that when working under known conditions and at a given pressure of steam, there is a certain ratio of expansion which gives highest efficiency, *i. e.*, least expenditure of fuel in proportion to work done. It has also been long known that the most economical ratio of expansion, all things considered, when studied from the commercial standpoint, is to be determined not simply by studying those conditions which affect the efficiency of the engine, but by consideration of all the elements of cost of steam-power, including first cost, interest on capital expended, wear and tear, and running expenses for fuel supply and management.

In the design of an engine it thus becomes necessary, if the designer would consult the best interests of the purchaser of the engine, first to determine as best he can what is likely to be the cost of each series of engines of the style proposed and of graded sizes; next must be determined the cost of power in weight of steam used in each of these engines at various pressures and ratios of expansion; finally, by comparison, he must select that engine which at the least ratio of expansion, all things considered, will give the required power.

When the steam pressure and the cost of power in steam consumption are known, it becomes possible to determine the best point of cut-off. But no engineer has yet been able to say with certainty what, in any given case, will be the ratio of expansion giving highest efficiency, or what, at any given rate, will be the probable expenditure of steam or fuel. The writer has endeavored, in an earlier paper,* to show what are the causes of this uncertainty,

* "On the Ratio of Expansion at Maximum Efficiency," Trans. Am. Soc. Mech. Engs., 1881; Journal Frank. Inst., May, 1881.

what determines the most efficient adjustment of expansion, and what, on the whole, have, in his judgment, proved to be the ratios of expansion giving maximum efficiency, and, finally, what efficiency may probably be anticipated in cost of steam and of fuel in good engines, adjusted for maximum efficiency.

When these points are settled the engineer may, by a proper use of the factors, ascertained as above indicated, determine what is the best ratio of expansion to adopt, or rather what is the best size of engine for the case in hand, and what the best type of valve gear and regulating mechanism. The size and kind of engines are therefore determinable from a knowledge of conditions, which are partly physical and incidental to construction, and partly commercial.

The solution of this second and broader problem may be effected either by a tentative process of trial and repeated estimation, or by an approximate, and for some cases—*e. g.*, where cylinder condensation is reduced to a minimum, as in efficiently jacketed or in fast-moving engines, or at low rates of expansion—nearly exact method, first indicated, so far as the writer is aware, by Professor Rankin,* who applies to the case one of those beautiful graphical constructions, in the devising of which he was so ingenious and successful.

This method has been studied and has been applied to representative examples in recent practice in steam-engineering by Messrs. Wolff & Denton,† who have shown that the commercially profitable grades of expansion are ordinarily restricted to a very narrow range, and always somewhat less than the ratio for maximum efficiency.

Having, then, fixed upon the size of engine and ratio of expansion, it is evident that this ratio of expansion should generally be kept invariable, so long as the steam pressure remains unchanged.

The usual changeable condition with a given engine is the demand for power, and to meet this variation it becomes necessary to adopt some method of regulation. The simplest forms of regulating apparatus usually consist of a "fly-ball" governor set to operate a "throttle-valve" or other kind of regulating valve; the most usual method of regulation with the better class of engines is that adopted by Corliss,—the attachment of the governor to the

* Phil. Magazine ; Trans. R. S. E. ; Theory and Practice of Shipbuilding ; Miscellaneous papers.

† Trans. Am. Soc. Mech. Engrs., 1881 ; The American Engineer, 1881.

expansion gear in such a manner as to cause a variation of the ratio of expansion, adjusting the point of cut-off to the demand for power. This latter is the most sensitive regulating mechanism yet devised, and where the variation is small is very effective even at low speeds. The writer has counted the revolutions of a Corliss engine making about sixty revolutions per minute, with steam at 90 pounds by gauge (7 atmospheres), and found a variation of but two revolutions per minute when the whole load was thrown off or on, the minimum being about 35 horse-power (driving shafting) and the maximum about 150.

Since, however, the proper ratio of expansion for the engine when once installed is determined mainly by the steam pressure, and since any variation from that point is usually productive of reduction of efficiency, it would seem that the ratio should be fixed at the best proportion for the steam pressure adopted and never changed. This being the case, the question arises, how shall regulation be effected? This adjustment of a throttle-valve by the governor is inadmissible, as it involves variation of the steam pressure in the steam chest and consequently reduced efficiency; the steam and expansion lines must be permanently fixed for all loads.

It becomes at once evident that any allowable system of regulation must now affect the back pressure or the cushion line. To throttle the exhaust by the action of the governor would undoubtedly give a means of regulation, but a costly one; since any increase of back pressure during the exhaust would involve serious increase in the amount of rejected heat and of waste of power.

It then becomes evident that the only admissible plan is the variation of the net power of the engine by an alteration of the compression line. This is done where one very well-known and generally used valve-gear is adopted—the Stephenson Link Motion. When the link is down the ratio of expansion is determined by the lap and lead, and is usually not higher than $\frac{4}{5}$; as the link is raised this ratio is increased, and with this change of the steam line occurs a simultaneous alteration of the point of release and of closure of the exhaust passages, resulting in increased compression. This double effect gives the indicator diagram a peculiar modification of form, familiar to engineers who have taken cards from the locomotive or the usual type of marine engine. The smoothness of working of such engines when running with high steam and a raised link has probably been observed by all experienced engineers, and it may not

have escaped notice that under such conditions the expenditure of steam is often so low as to indicate some source of economy other than simple change in the ratio of expansion.

The writer, at least, when in charge of naval steam machinery during the war of 1861–65, was led to suspect a gain from this distribution of steam, which could only be attributed to what was considered excessive compression.

His attention has recently been called to this matter again by the interesting results of a series of experiments made upon a large engine fitted with variable expansion gear. The valve motion is so arranged as to permit adjustment of compression without change of either steam, expansion or exhaust lines. The results will be reported in a later paper. It is only necessary here to state that a decided gain is found to follow the adjustment of the compression to a far higher ratio than is indicated as best by the simple geometrical conditions usually studied and generally taken as those determining the proper ratio of compression. This beneficial effect of a high ratio of compression has been attributed by the writer to the action of the compressed fluid in heating the passages and the cylinder head and piston, thus checking to a very great extent that initial cylinder condensation which is the greatest source of avoidable waste in nearly all engines.

It may be asserted that the best compression, where no such transfer of heat occurs, is not far from that which makes the ratios of expansion and compression equal, and the engineer will usually set the exhaust valve to close at the point corresponding to maximum expansion. For the reasons just given, however, and as shown by direct experiment, maximum efficiency is obtained with higher ratios of compression, and what would have been considered excessive cushioning gives less loss than equal variation from the point of cut-off giving maximum efficiency. As compression is increased, the area of the indicator diagram decreases, and the work developed in the engine becomes less.

It would seem, then, that we have here an admissible method of regulation, and one which should be, on the whole, that best fitted to give high efficiency, since any excess of work of compression results simply in the transfer of heat back to the steam side. The steam-engine should, therefore, be worked with a fixed cut-off,*

* The writer has devised methods of automatic readjustment of the ratio of expansion when variations occur in the steam pressure, which methods would in the case here taken replace the usual adjustment by the governor.

so attaching the governor as to determine the point of closing of the exhaust valve—in other words, making the cut-off operate on the exhaust side, the ratio of compression being determined by the governor—instead of attaching the cut-off mechanism to the steam valves. Properly constructed relief valves will prevent all danger from the influx of water with the steam, an accident which, however, should never occur where provision is made for securing dry steam. With exhaust ports beneath the cylinder, drainage is rarely imperfect.

In slowly moving pumping-engines it has sometimes been found beneficial to extend compression until boiler pressure is exceeded, and the writer has indicator diagrams taken from such engines in which the compression line crosses the steam line before the end of the return stroke has been reached. He has, as probably has every engineer who has been accustomed to handle locomotive or marine engines, often set the link motion so as to give such high ratios of expansion and compression as to reduce the card to a comparatively narrow band without perceiving the slightest evidence of objectionable loss of smoothness of working, and with decidedly improved efficiency. It seems to the writer doubtful whether, in practice, objectionable or "excessive" compression ever occurs in such cases, and the advantages of this method of regulation, and of securing a lessened variability in the ratio of expansion, would appear to be decided and to be obtainable without meeting with serious difficulties.

The plan is probably not entirely a novel one, and may have suggested itself to many engineers independently; but no attempt has previously, so far as the writer is aware, been made to determine its advantages in actual work. The writer indicated this as the proper method of adjusting expansion some years ago,* and has since had it presented to him by other engineers with whom the thought was also original.

Where the plan here suggested cannot be adopted conveniently, maximum economy of steam should be obtained by an expansion gear, in which, as in locomotive valve gear, increased expansion is accompanied by increased compression, but without that serious throttling along the steam line which usually characterizes the distribution by the link motion.

The best among existing forms of valve gear should be, if

* History of the Steam-engine. D. Appleton & Co. (International Series), New York, 1878, p. 473, foot-note.

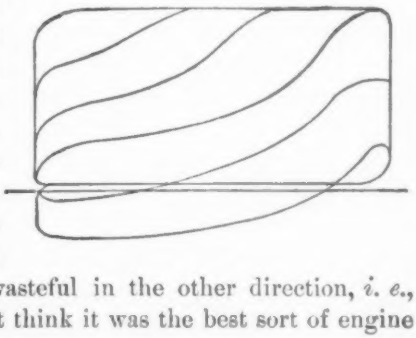
judged from the standpoint here taken, that which—combining a variable expansion with a variable compression—is also capable of prompt and exact adjustment by a sensitive and efficient governor.

The economy to be expected from the suggested change in methods of regulation of the steam-engine will evidently be dependent upon the manner of operation of the engine. Where engines have a nearly invariable load, and when they are well adjusted to their work, the advantage would probably be found inappreciable; but in cases in which the engine is much too large for its work, or when the demand for power is very irregular, as in rolling-mills or in rough weather at sea, and where cylinder condensation occurs to a great extent, the increased efficiency may be found to be very considerable. The gain by decreased internal condensation will, perhaps, often be found to be an item of no small importance.

DISCUSSION.

Mr. Stirling: I am glad this subject has been introduced, because it is one of great interest just now. My experience leads me to believe that we have gone altogether too far in the direction of automatic cut-offs. By means of cards I can show some points in connection with this which I found in my experience with a large 42″ × 42″ Corliss engine. About forty or fifty puddling furnaces were attached to it. The work was very irregular—about six sets of rolls, and sometimes two pieces in each set of rolls, and sometimes nothing at all. In tracing indicator-diagrams it would first mark a full-stroke card. Then the cut-off would begin to get gradually shorter and shorter, as in Fig. 121, showing that a great deal of the work was being used in pulling the engine back. It cannot be said that this card was an economical one. Very frequently during the working of the engine will be found on the indicator a full-stroke card, and one of the most wasteful possible; and then perhaps will be traced expansion-cards wasteful in the other direction, *i. e.*, from pulling back. I do not think it was the best sort of engine

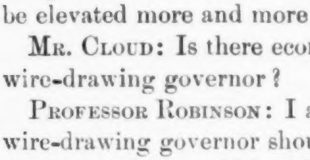
Fig. 121.



for the work, because there was no object in having it run so very regularly. I think this matter which Professor Thurston has introduced is one which should occupy our attention. I do not think we have got to the bottom of it.

PROFESSOR ROBINSON: As I have studied the question of wire-drawing, I think much less credit is given to the ordinary wire-drawing governor than is due to it. The steam which escapes through the wire-drawing valve must be superheated by that wire-drawing operation. Whenever steam expands without doing work, it must retain its intrinsic energy according to theoretical considerations, and this is found practically to have been sustained. Steam which escapes from an orifice is found never to deposit moisture on anything that is laid in its course—under ordinary circumstances at any rate—indicating a superheating of the steam; that is, it is carried far beyond the point of condensation. It has more heat at this particular instant of expansion than is necessary to hold it in a saturated condition. Now, the steam which passes through the wire-drawing governor-valve must pass into the cylinder in this condition, and although the first portion of the diagram is lowered, the latter part is elevated. To indicate, in Fig. 122 the portion B is recovered from the steam by wire-drawing, while along the space A, the pressure is lowered from beginning of stroke to X by the wire-drawing governor. If this steam were admitted to the point of cut-off, and then expanded, the expansion-curve would be elevated more and more from the point of cut-off forward.

FIG. 122.



MR. CLOUD: Is there economy connected with the use of the wire-drawing governor?

PROFESSOR ROBINSON: I aimed to have it understood that the wire-drawing governor should have more credit given to it than is sometimes done. Although I think it is extravagant, yet I think it is not quite so extravagant as we might imagine it to be. When the steam is wire-drawn, say from 50 to 25 pounds pressure, or from 100 to 50 pounds pressure, there will be retained the full energy, theoretically; and if we were using this steam in a condensing engine, there would be but a very slight loss, so that the wire-drawing governor would be a far better governor for a condensing than for a non-condensing engine.

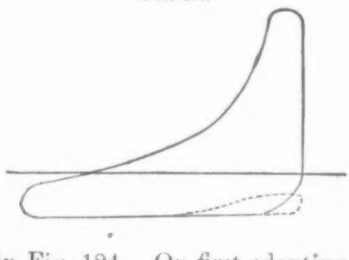
MR. STIRLING: I will show some other indicator cards that I

have got lately from condensing engines with wire-drawing governors which might be of interest on the blackboard.

THE PRESIDENT: Everything is of interest in this connection.

MR. STIRLING: This matter is very interesting to me because I have been into it lately. I think Professor Robinson is getting down to a pretty safe basis when he opines that with a condensing engine the wire-drawing governor is about right. The engine from which these cards were taken was a slide-valve engine of the very best construction. We put on this engine what is known as the Farcot cut-off. It did not call for the use of an additional eccentric at all. The cut-off is made by a plain plate of metal on the back of the valve. The duty of the engine is hoisting coal from the mines. The card printed when the coal is coming up is a good deal like the full line of Fig. 123. When the car is on top of the breaker no work is wanted—the object being only to keep the speed of the engine down, and that is done by shutting the throttle. In fact it could not be shut tight enough to keep the engine at a moderate speed. The card printed with the throttle shut is shown in the dotted portion of Fig. 123. That is the card when the engine is running light. I do not see how that engine can be made to work more economically.

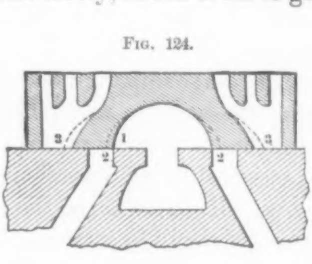
FIG. 123.



DR. GRIMSHAW: How about the compression?

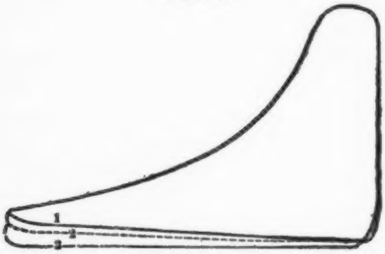
MR. STIRLING: We do not get any great degree of compression. You will see how this is accomplished when I explain. The valve and its seat are shown in Fig. 124. On first adopting this form, I found that it did not exhaust freely, and in order to get rid of the steam and establish a vacuum, I cut away the exhaust edges at 2 and 2, and that enabled me to make the difference shown in diagram 2 of Fig. 125, and I immediately saw I was on the right track. I added pieces of steel at 3 and 3, and the effect was to produce a card like diagram 3 of Fig. 125. In my judgment that is more economical than it would be, if the card remained like diagram 1 of Fig. 125.

FIG. 124.



PROFESSOR THURSTON: Comparing the two methods of regulation, I think it would be found, on consulting anybody who has had experience of the two methods, that we may obtain a large increase of efficiency by the use of the automatic adjustment of the point of cut-off. I can speak rather feelingly on the subject, because some twenty years ago, when Corliss was building his engine, our people were building an engine on the other plan, and Corliss beat us out of the market entirely. This to a certain extent was no doubt owing to the fact that he was a little more careful in building his engines, because he was trying to work into the market; while we had held the market a long time and were getting careless. But behind that was the fact that he determined the point of cut-off by the action of the governor. We tried to adhere to our old methods and to our old design, but we had to give them up; and the upshot of the whole matter was that the introduction by Corliss of that method led to a complete revolution in our methods of manufacturing steam-engines, and that revolution simply came from its higher economy, as a matter of course. But there is no question I think to-day in the mind of any engineer who has had much experience, that it is easy in any given case to determine the point of cut-off that shall be the best point of cut-off for that case, according to the amount of work done, the cost of fuel, and expenses, and the style of engine. Usually it is governed pretty largely—it is governed perhaps exclusively by the mean amount of work to be done, and by the pressure of steam. That point found, it should be retained. Now, if with an unjacketed engine we set our engine at a quarter, and then vary the speeds at different times from one-tenth to nine-tenths, we will find that at nine-tenths we use about as much steam as at one-tenth. If, however, we regulate by means of the compression or exhaust, we do not meet the same losses. If we can make the cut-off uniform, and then diminish the area of the card by means of the compression line we shall have no losses from the variations of the point of cutting-off, and obtain some incidental advantages. I have not thought enough of this matter to be very confident as to the desirability of making such a

FIG. 125.



change as is presented here, as a suggestion, and as indicating a direction in which, probably, economy is to be obtained. The action of the locomotive is an example of that method. Whenever an engine is running at high speed we shall find it making a curve on the expansion line which is a very good expansion curve.

With a card like that of Fig. 126 we have a variation like that of the locomotive, in which we have a very good expansion line, but on the lower side of the card we always find a large amount of compression. Thus in spite of all the disadvantages under which it labors, we find in the locomotive a much greater economy than would be expected, especially when we consider the low efficiency of its boiler.

FIG. 126.

MR. PORTER: The method of regulating the steam-engine, which I understand to be presented in Professor Thurston's paper, is that of having a fixed point of cut-off and varying the compression. There seem to me to be two objections to that mode of working the steam-engine. One is that the range of variation would be very slight; only a small variation of resistance would be provided for. In many cases we should have sufficient, probably. In cases of mills which run with an almost unvarying load all day, a variation of ten or twenty per cent., possibly sometimes five per cent. is all that is necessary to be provided for in practice. But engines cannot be made with reference to that requirement only, they must be made with reference to whatever requirements may arise in any case whatever, and very generally the range of variation is quite extreme, so much so, that the variation of the compression line would not by any means provide the requisite variation in the power exerted by the engine. The use of the throttle valve, if the point of cut-off remained fixed, would have to be adopted, unquestionably, to supplement this means of adjustment. The locomotive, as Professor Thurston has observed, gives us a very remarkable illustration of the value of a varying compression line, when combined with the varying cut-off. Where the throttle valve is not used at all, and the engine-driver controls the engine entirely by the quadrant, he may set his cut-off at an exceedingly early point in the stroke, and then the compression will commence at the mid-stroke, in fact, a little before the mid-stroke, and if the waste-room is not excessive, the expansion curve and the com-

pression curve are separated but by a very narrow interval, so that the inclosed area of the diagram is a narrow band; and under those conditions the engine works with the very highest economy with which an engine doing nothing at all can work.

There is another objection which occurs to me against the employment of this mode of regulating an engine, and this is that it varies the point of compression, while in point of fact, for the attaining of the greatest economy, we would like to have always all the compression we can get. However much the point of cut-off may be varied by the action of the governor, the compression, starting from the same line—in a non-condensing engine the atmosphere—will necessarily rise to the same point if the exhaust action is invariable—if the exhaust valve closes at the same invariable point—and for the attainment of the highest economy it is desirable that this closure should be effected early, so that if there is a small waste-room to be filled, the compression line may rise to the boiler pressure, and also that the interior surfaces of the cylinder, piston, heads, ports, valves, etc., may all have their temperature raised, as the pressure cannot rise without the temperature of the surface rising also to a corresponding degree. That is one advantage which the non condensing engine undoubtedly possesses. With a fixed point of closing, and a small percentage of waste-room, so that the compression line can rise to the boiler pressure, I suppose that the best way of regulating the engine, the most economical and advantageous in all respects, is by varying the point of cut-off.

I must say that I should be very much interested personally to become acquainted with the mechanism to which Professor Thurston alludes, by which the point of compression is varied, assuming that the point of release is not also varied.

Professor Thurston: I referred there to a form of valve that is used a good deal among the mines in the West—O'Neill's, I think, is the name. There is an opportunity for adjustment of every part. The experiments referred to were made on such an engine, the point of compression being determined without any change of expansion line or exhaust line. I have drawings of it, and some day when Mr. Porter is in my study I shall be glad to show them. It is the only case, however, in which I have seen the thing done, and I shall be happy to get the results of experiments with these engines and present them to the Society. I think they will prove interesting, not in this connection only, but in a number of ways.

Mr. Porter: I suppose that if the compression starts from the line to which the expansion has already been carried, we may consider the confined steam to behave as a constantly acting spring —that steam which fills the waste-room giving out by its expansion exactly the force that is required to compress it back again to the density of the boiler pressure; so that no steam whatever is used to fill the waste-room, and the surfaces of the metal have their heat restored to them. While in a condensing engine the early closing of the exhaust valves can do no harm, in a non-condensing engine it certainly is always advantageous. I think that a fixed point of closure, and the earliest point practicable, is, from economical considerations, the best.

XLVI.

THE LATEST METHODS OF SUBMARINE TELEGRAPH WORK.

BY THOMAS WHITESIDE RAE, C.E., NEW YORK CITY.

This branch of engineering is of comparatively recent growth, and at first glance may seem hardly to be mechanical in its characteristics. The popular impression among those who devote any thought to it at all, is apt to be that it only involves the functions of the navigator and seaman. The real case is that only well-built and accurately working machinery, designed by the light of experience gained in all kinds of weather and over all varieties of ocean bed, writes successful submarine telegraph work upon the list of engineering possibilities. It is interesting to trace how entirely, from the first step, the prosperous conduct of an enterprise of this nature depends upon the perfection of the mechanical appliances employed in it. For example, the construction and laying of a submarine telegraph must be regarded from two points of view, viz., as an engineering problem, the sagacious solution of which stimulates professional pride, and as a commercial question, whose satisfactory answer is the only justification of the time, money, and labor devoted to the undertaking. About the most important factor in the financial success of a submarine telegraph is its capacity for business, or, in other terms, the number of words it can transmit in a given time. This fixes its revenue-producing power. Statistics show very distinctly the ratio between the foreign com-

merce of a people and the extent of its telegraphic correspondence, and this is the source from which must come the fund that shall cover the cost of maintenance, pay interest on the original investment, provide for complete renewal in twelve or fifteen years at the farthest, and afford satisfactory dividends to stockholders. The capitalist knows that trade to a certain amount between two countries means the interchange of so many words by telegraph, for which so much money will be paid. The first duty of the engineer is to decide how great a capacity of transmission the submarine telegraph—more conveniently styled cable—may have.

Rapidity of transmission means size; size means weight and bulk; these in turn, imply ships of certain tonnage, and it becomes apparent that not a step in the enterprise can be safely taken until the proposed route shall have been made the object of an exhaustive survey.

When the length of the longest uninterrupted circuit is known, and the greatest depth of water, the profile of the ocean bed and its chemical constitution along the projected course discovered, the engineer can predicate, with reasonable exactitude, how much cable shall be of a certain tensile resistance and how much of another, and whether or not any portion must be guarded from destructive action of the surface on which it will lie. These essentials to durability being definitely decided, it becomes possible to indicate the largest insulated conductor which, in combination with the other constituents of a complete cable, the weight-carrying and stowage capacity of the ships available for the work renders permissible. The capitalist then may judge what the prospects of commercial success are.

To arrive at this stage, resort must be had to copious and accurate deep-sea sounding, and thus, at the threshold of the enterprise, the need of mechanical appliances is felt, and it is fully met by the apparatus of Sir William Thomson, Professor of Engineering of the University of Glasgow. This instrument, devised as the recreation of a yachting cruise, is so infinitely superior to anything else of the kind as to put them out of court entirely. It has been materially improved, in detail, by Lieutenant-Commander Sigsbee, of the United States Navy, but perhaps has reached its fullest development in the form built and used by the I. R. G. P. & Tel. Co., of London. The fundamental principle of the machine is elimination of friction of the sounding line, which is the great obstacle to the vertical descent of the weight,

to receiving warning of the bottom having been reached, and to the recovery of specimens of the ocean bed. So long as flax or hemp was used it was found that, even when reduced to the smallest practicable dimensions, the point was speedily reached where the frictional resistance of its surface would neutralize the gravitation of the heaviest sinker, and that stratified currents swept the line in the most diversified and incalculable sinuosities. Even the ingenious devices of the late Lieutenant Brooke, U.S.N., for detaching the sinker when bottom was touched, thus sparing the line the strain of lifting it to the surface, fail to overcome this trouble; and the expedient of dispensing entirely with the line—as in the Morse bathometer, which sinks freely to the bottom, detaches its weight and returns to the surface by its own buoyancy, recording the pressure (and thus by inference the depth) it has been subjected to by the amount of mercury forced from a compressible vessel through a minute tube into an incompressible one—only introduced another embarrassment, as the distance and direction of the point of disappearance of the apparatus from the point of its reappearance was—owing to stratified currents—a matter of pure chance. Sir William Thomson secured comparative directness of descent by using small steel wire, which also reduced frictional resistance to a minimum, and obtained unmistakable and instantaneous notice of the bottom having been reached by keeping the sinker so nearly in equilibrium that the slightest arrest stopped the running out of the wire. This is accomplished by accurately balancing with brakes the weight of the wire paid out, and, as bodies falling freely through water are found to very nearly describe equal spaces in equal times, the sinker can do this undisturbed by any increase of weight from the wire paid out. The law here cited affords an excellent check for indicated soundings.

Figs. 127 and 128 show the essential features of the machine in question. They consist of a frame bearing a galvanized iron reel R, of steel sounding wire, removable at will from the shaft to which it is keyed. To one side of the reel is attached a grooved brake-wheel, B, which is enveloped with a cord whose one end is secured to the frame and the other to the brake-lever. The guide-wheel G is a decided improvement upon the original machine, having an oscillatory motion, which permits the wire to trend in any direction, while paying out or reeling in, with perfect impunity. It and the reel are tangent to the same straight line, and the bent

arm which carries its journals allows a vibratory motion in the bearing at a right angle to the reel's plane of revolution without disturbing this relation. The wheel is counterbalanced (see C) with the least possible preponderance, to preserve its verticality, and yields so readily to the slightest pull of the wire that soundings to a depth of fifty fathoms may be taken from a vessel moving at a considerable speed.

The invention of the brake for balancing the weight of wire paid out is claimed by the author and operates in the following manner: Upon the shaft, S, which bears the reels, is a worm which engages with a gear-wheel, E, keyed to the end of a long fine-threaded screw, F, and carried by the composite brake-lever shown in the appended sketch, Fig. 129. Revolution of the reel-shaft either way causes this screw to turn—but without longitudinal motion—in its supports. Riding upon this screw is a weight, W, made in the form of a hollow box, for accurate adjustment, which is accomplished by putting in or taking out shot. It is so shaped as to allow of its coming directly beneath the brake-fulcrum, or even passing beyond it and becoming neutral, or of contrary effect if desired. The weight of the steel wire employed for sounding, which is of the class prepared for piano-makers on account of its exceptional tensile strength, is about nine pounds per hundred fathoms when submerged, and the screw-thread is cut and worm-wheel so proportioned as to cause the proper increase of brake-leverage needed to exactly balance the regular increment of weight. Another weight, K, may be observed at the extremity of the brake-lever, also capable of adjustment like the travelling weight. This is to balance the constant weight of the sinker. The method of procedure is to place the travelling weight in a neutral position, and—allowing the sinker (a spindle of cast iron of about thirty pounds weight, with a large enough longitudinal passage therein to allow of the specimen cup slipping through it when the sinker is detached) to depend from the reel and just submerged—to vary the fixed weight, K, until there is only preponderance enough to allow the reel to turn. The varying condition of the journals and lubrication of the machine render this adjustment necessary. The travelling weight preserves this status very exactly, as the friction of the sounding-wire is practically *nil*, and the slightest checking of the descent of the sinker instantly stops the reel. A counter recording the revolutions of the wheel then gives the depth.

In Sir William Thomson's original apparatus the method of balancing the wire paid out by increasing the brake-pressure was most elementary, being accomplished by hanging small weights of definite size upon the brake-strap at regular intervals of time.

There is nothing peculiar in the device for detaching the sinkers on touching bottom, as it has not varied materially from the days of Berryman and Brooke, the naval hydrographers; but a very effective and inexpensive specimen-cup has been added, consisting of a cylinder with its upper end closed,—with the exception of one or two small perforations,—and its lower covered with a diaphragm of rather stiff india-rubber, slightly concave externally and crossed with two diametric cuts at right angles to each other. This perfectly meets the exigencies, which are to have a cup sufficiently mobile to open on touching soft ooze, and yet tight enough to prevent its contents being washed out in the ascent. The rapidity and accuracy of this machine is illustrated by a series of soundings taken between Marseilles and Algiers in September, 1879. The final route was across zigzags (shown in Fig. 130), which were made for the purpose of ascertaining the inclination of the bottom athwart, as well as along, the line of the cable.

These soundings average 1150 fathoms in depth, and were taken in six days, during which the ship ran nearly 700 miles, and for one-third of the time continued work through a *mistrale* in the Gulf of Lyons with no greater loss than about 7000 fathoms wire and some twenty-five sinkers and specimen cups. It is necessary to say at this point that some very good authorities prefer to recover the sounding-weights at each cast, and this was the practice on the occasion in question. Samples of bottom were obtained in all cases except two, exclusive of the occasions when the wire broke. The reeling-in was performed by a deck-engine connected to the apparatus by a rope belt. By old methods this work would have occupied a fortnight, while possessing none of the authority and precision of this. The method of preserving the sounding-wire is to immerse it in a bath of caustic soda, which keeps it bright and unoxidized for two or three years. Each reel of wire, containing 4000 or 5000 fathoms, is used for about twenty-four hours, and then returned to its bath, and a fresh reel keyed to the shaft of the machine, which is contrived with a view to facilitating the operation as far as possible.

From the knowledge obtained with this most effective apparatus, the engineer may now prescribe intelligently the proportions of

the cable and the quantities of the different types—that is, the heavy "shore-end" in the vicinity of anchorages, and the "intermediate" for less exposed localities—and is competent to indicate where special precaution must be taken against chemical destruction of the cable-armor by the ocean-bed. This is usually accomplished by serving with jute and coating it with a mixture of tar and silica, which also defends it from marine insects, but it is found to be difficult to lay the cable with this covering intact. The discovery of some alloy, not so costly as to exclude it from the list of useful metals, that should show a rate of oxidation notably less than that of iron, yet possessing all its tensile strength and ductility, would be the touchstone to commercial success in submarine telegraphy. The conductivity and insulation of submarine wires is all that can be desired, and once safely laid are indestructible while the armor lasts, but this failing through corrosion, currents and marine insects work their will on the delicate conductor, which of itself is too weak to be lifted to the surface for repairs. Hence it is a canon of submarine telegraph finance that provision must be made for complete renewal about every twelve years.

The construction of a profile of the ocean-bed from these soundings by the usual method is too familiar a process to be noticed here, but recent experience has established the wisdom of plotting it with a common vertical and horizontal scale, for the following reason. Of course this demands something very like an old-fashioned panorama to be at all manageable, but it is not difficult to devise. Cross-section paper of the proper scale can be procured of any length, and by attaching a common drawing-board to a standard having rollers at each end, with crank-handles, the lengthy diagram can be conveniently used. By unrolling it with one crank and rolling it up with the other, the point over which the ship chances to be can always be kept on the drawing-board.

The use and convenience of this is readily illustrated. It is frequently noticed, in laying a telegraph-cable, that the velocity with which it leaves the vessel varies decidedly without corresponding change of the ship's speed, rendering necessary perpetual adjustment of brake-power to preserve the ratio of "slack,"—as is termed that excess over the linear distance which is needed to insure the cable's touching ground throughout its path over submarine hills and dells,—which the situation prescribes. It is distasteful to the engineer to waste valuable cable, yet if the depth is increasing rapidly it is imperative that the percentum of slack be augmented,

and *vice versa.* The position of the ship may be readily computed at any moment, but the depth of water at that point by no means reveals that at which the cable is taking ground. This, in a depth of two thousand fathoms, with the ship moving at six knots per hour, may be four miles astern, and is that to which fluctuations in the speed of the outgoing cable are due. Now, from the fact already cited, that bodies sinking in water assume, after the first two or three seconds, a uniform downward velocity, the direction of a cable between ship and bottom, while being laid, is approximately a straight line, and an empirical formula, given by Latimer Clarke and Fleeming Jenkin, renders it possible to calculate the angle it makes with the surface for any given cable. If, then, a bevel-gauge of suitable dimensions be set at the angle proper to the cable in question, the engineer, by applying it to the true-scale profile, already described, easily finds the vertical component of the stress which he must control with his brakes. The bevel-gauge cuts the ship's position on the surface, and the sounding over the point where it cuts the bottom, measures the downward pull of the cable at the ship's stern. So that when the outgoing cable gathers speed, while the ship maintains a steady pace, and there is a doubt whether or no the brakes are not becoming polished and greater force should be applied to them; or, on the contrary, when it passes overboard at a slackening rate, and the fear arises that dry brakes or heating journals may be the cause, a recourse to the bevel-gauge and profile will show indubitably that the cable is taking ground later or earlier, and the necessary measures may be adopted.

At this point it seems proper to make reference to an apparatus which, in combination with the device just described, enables the percentum of slack to be regulated much more accurately than could be done by former methods, even with the closest personal supervision of the engineer, whom it relieves of this most exacting and onerous demand upon his attention. The old routine was to order the man stationed at the dynamometer to keep a definite strain upon the outgoing cable, which the judgment of the engineer decided to be the proper one to maintain a uniform rate between its speed and that of the ship. The revolutions of the brake-drum were taken at regular intervals, more or less close, and compared with the reading of the log, and with a result rarely very satisfactory. The difficulty also of keeping the prescribed strain upon the cable was very great in all except the calmest weather. The new instrument, called the strophometer,

consists of a dial in full sight of the man stationed at the dynamometer, connected by a driving belt or cord with the brake-drum, the number of whose revolutions per minute are indicated by the pointer, and evidence of the instrument's working giving by the sounding of a small bell at each revolution. The normal ship's speed being about one hundred fathoms per minute, and the circumference of the average brake-drum about three fathoms, the calculation of "slack" becomes a question of revolutions, which the dynamometer-watch can easily maintain as ordered—with the strophometer appealing constantly to his eye and ear—by means of the jockeying which experience teaches him.

One simple but most important change in the paying-out machinery is that in what is termed the "holding-back gear:" otherwise it has only differed in detail from that which Everett designed for the "Niagara" when he assisted at laying the first Atlantic cable. The submerging cable is controlled entirely by the adhesion of several turns of it about a strong drum, keyed to the same shaft that carries one or more heavy, wide-faced wheels, against whose circumference wooden blocks are pressed by a belt, which is drawn tight by weights acting through levers; the details are unimportant here. A necessary adjunct to this was something to keep a slight pull upon the cable as it took the drum, that the full benefit of its adhesion might be insured. This was compassed by letting the cable pass over V-shaped grooves, in the peripheries of wheels whose axles also carried brake-wheels, of which the tension could be regulated at will. Other weighted wheels, called "jockeys," rode upon the cable, crowding it into the wedge-shaped channel, thus securing the necessary adhesion. Adjustment of these small brakes gave the moderate pull required for this purpose, but it was discovered that in a sea-way they became a source of danger. As the stern of the ship settled in the trough of the sea, the brake-drum would come nearly to a standstill; then rising with the next wave would cause it to revolve with extreme rapidity, imparting this motion to the holding back gear, whose momentum lasted until after the next slackening of the brake-drum, thus causing the cable to pass to it loosely, to the great danger of overriding or "rendering" upon it. The substitute for this is passing the cable between alternate quadrants, adjustable upon a horizontal bed-plate (see Fig. 131), and it meets every requirement.

The method of landing cable-ends from ships depends largely

upon the region in which it takes place, but the method adopted by the I. R. G. P. & Tel. Co., of London, is of almost universal application and worthy of notice. This portion of the work is always done first; the heavy shore-end cable and intermediate type being put down at both termini, and buoyed, usually with a strong grappling rope, in a depth of water into which the cable-ship may safely run. She then splices on the deep-sea type at one buoy and runs for the other. The ship approaches the land as near as safety warrants, and, having anchored, sends ashore a large spider-wheel, so called, and one or more mushroom anchors. These are buried to a suitable depth, and the spider-wheel secured to them in such a manner as to revolve freely. A line is then passed out over the sheave at the ship's bow, carried ashore in boats, rove through the spider-wheel, and its end brought back to the ship's stern, where it is made fast to the cable depending from the stern-sheave. The powerful picking-up machine in the forepart of the ship now begins heaving in the line, and, as it comes aboard, cable is payed out at the stern of the ship. The sketch, Fig. 132, shows the entire operation.

It is necessary, of course, to sustain the weight of the cable payed out, and this is effected by attaching barrels to it at definite intervals as it goes overboard. While perfectly effective, these are clumsy to handle and most cumbrous on shipboard, and the author substituted for them, at the laying of the Marseilles-Algiers cable of 1879, india-rubber spheres of three feet diameter, inflated by a small air-pump when needed. These answered every purpose, were most convenient of manipulation, and were capable of the most compact stowage. This operation can be easily carried on at the distance of a mile from shore, and possibly further.

The introduction of the electric light is all that remains to be adverted to. This might seem unworthy of notice to one who had not undergone the wearing anxiety of night-work on a stormy sea in laying submarine telegraphs. The exhausting influence of hour after hour of intense watchfulness, with every sense strained to make good the handicapped eyesight and catch timely warning of aught amiss with the cable-tanks, the intricate assemblage of deck-machinery, and the elusive indications of the electrical test-room, is something that must be experienced to be appreciated. The flood of light poured over the deck by electricity lifts half the burden.

The preceding *résumé* has adhered strictly to its title, being little more than a recital of improvements in the methods of subma-

rine telegraph engineering that have been taught by the experience of recent years. The essentials differ little, if at all, from those adopted in the youth of such enterprises, and only afford another proof that the daring spirits who tied continents together beneath "the astonished sea," did not win success by a happy chance. It was faith, patience, pluck, close thought, and hard work, which never fail of their reward. The direction in which further improvement must be sought is with cables themselves, and not with the methods of constructing or laying them. Durability is the great lack, and the promoter of this, in even a limited degree, cannot fail of a noble reward.

DISCUSSION.

Mr. Gordon: I would ask Mr. Rae if there is any difficulty in paying out the cable through those quadrants, such as destroying the coating of the cable.

Mr. Rae: That is exactly the objection to them, and yet it is a minor matter compared with the other difficulty of which I spoke, namely, the holding-back gear acquiring velocity enough to shoot the cable along and let the drum take it in a slack condition. This covering is very apt to be torn by the quadrants, but it is a difficulty so much less than the other that it is tolerated.

XLVII.

NOMENCLATURE OF MACHINE DETAILS.

BY OBERLIN SMITH, BRIDGETON, N. J.

That the nomenclature of machinery, and of the tools and apparatus with which it is constructed, is, in this country, in a state of considerable confusion, scarcely needs demonstrating. If we look from an international point of view, and include the other English-speaking countries, Great Britain and her colonies, the confusion becomes worse confounded. A reform is destined, in due time, to come, doubtless to be promoted in great degree by such societies as ours. This reform movement cannot be begun too soon, and should aim at giving brief and suggestive names to all objects dealt with—each object to have but one name, and each name to belong to but one object. A simple method of beginning such a reform

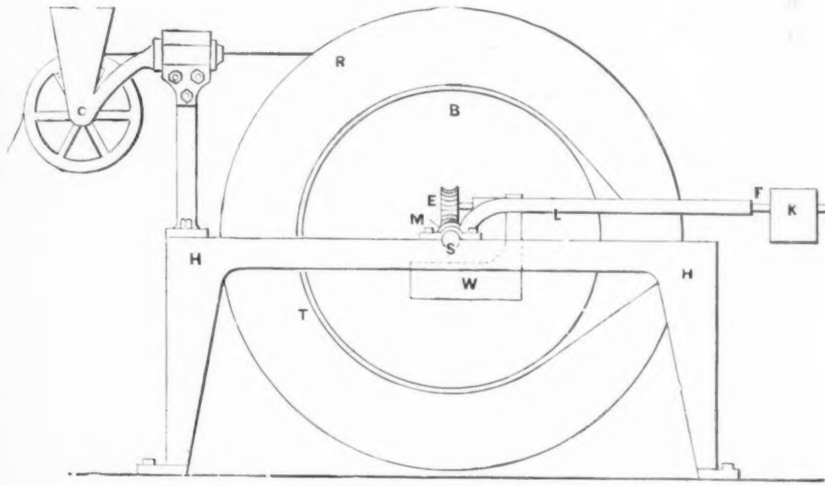


FIG. 127.—*Side View of Sounding Machine.*

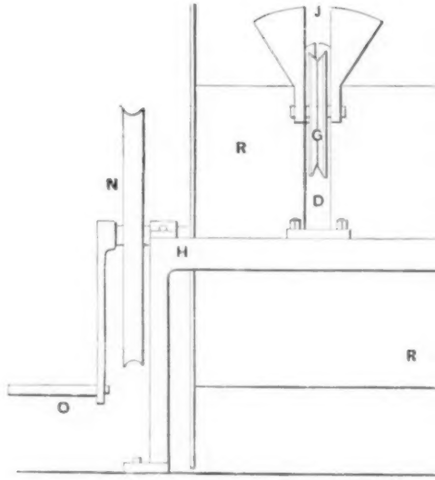


FIG. 128.—*End View of Sound*

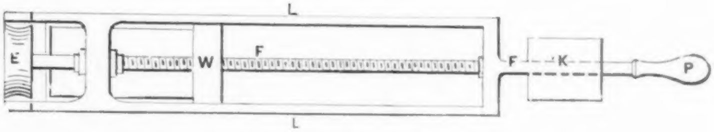


FIG. 129.—*Top View of Brake Lever shown in Fig.* 1.

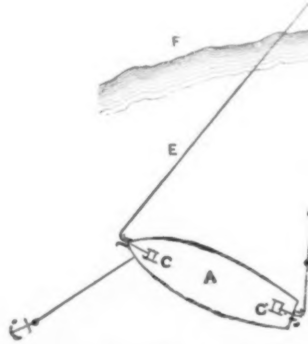
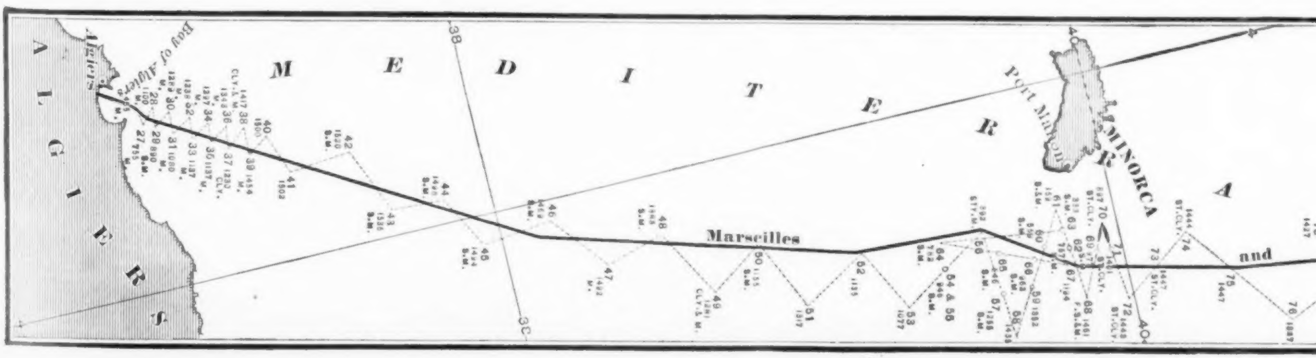
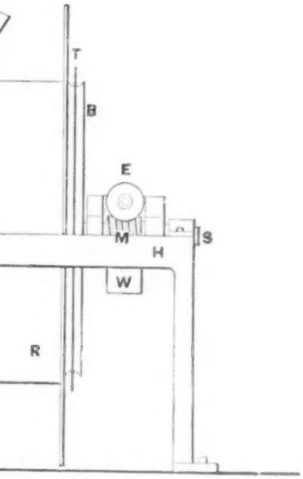


FIG. 132.—*Landing the Shore*





Sounding Machine.

hore End of a Cable.

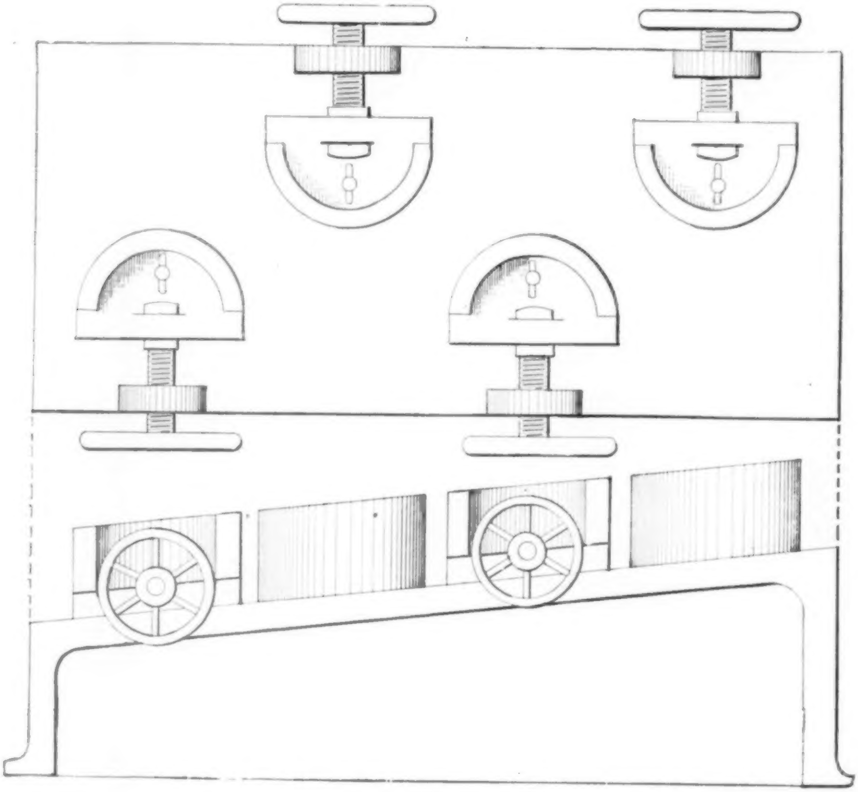


FIG. 131.

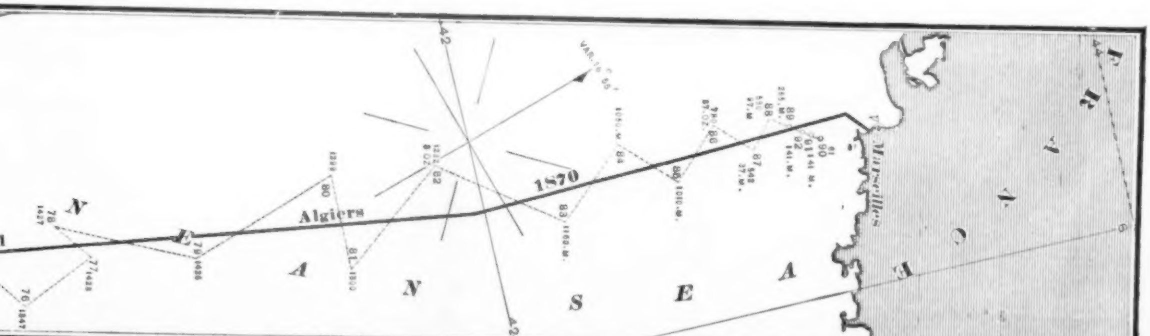


FIG. 130.

would be a common agreement among all our engineering schools to use each technical word in but one sense, and with no synonyms.

A lesser field of reform, and one which lies more particularly within the jurisdiction of individual manufacturers, is the comparative designation of a number of sizes or kinds of the same machine. There is now no common understanding whether a series of sizes shall be numbered or lettered from the largest down, or from the smallest up. The latter is undoubtedly the most natural and suggestive method, but usually becomes confused by want of careful forethought (when starting a series) in providing "gaps" for the insertion of future sizes. If a numerical series has been already started, and becomes commercially established, the only systematic way to insert new sizes (either at the beginning or through the middle of the series) is to use fractional numbers. This, though awkward in sound and appearance, seems to be the only means of suggesting the comparative size of the article by its name. The use of arbitrary higher numbers between the others is, of course, worse than no numbers at all. The use of a series of letters does not supply this fractional loop-hole of escape—the euphony of A-and-a-half, K-and-three-quarters, etc., being somewhat doubtful. Another method in much favor is the use of "fancy" names, such as "diminutive giant," "eureka," "firefly," etc. These are far preferable to confused numbers, as they are not intended to convey any ideas between manufacturer and customer, and admirably succeed in their purpose. All this is a very difficult subject to deal with, and one in regard to which we can scarcely hope for any exact system. We can but point out to manufacturers two general principles to be followed: 1st. Of leaving abundant *gaps*—that is, let a regular series run 10, 20, 30, 40, etc., instead of 1, 2, 3, 4, etc.; and 2d. Of using the smaller numbers for the smaller objects. The first is similar in idea to the well-known Philadelphia house-numbering system, which has worked so admirably in practice, and which has been copied by numerous other cities.

The two foregoing paragraphs are intended respectively as but casual allusions to the technical and commercial nomenclature of machinery in general. The subject is too elaborate to be treated at length in this paper, the main purpose of which is to set forth the results of the writer's experience in establishing a system of names and symbols for all the component parts, commonly called "details" of machines, or, in fact, of any manufactured articles.

That some such system is necessary, no engineer who has attempted to manufacture machinery by the modern system of duplicate (or approximately duplicate) parts, will, for a moment, question. The necessity for a specific name for each piece, which name is not, never has been, and never will be, used for any different piece of the same or any other machine, is evident, simply for purposes of identification. This identification is required mechanically at almost every stage of production. The name, or a symbol representing it, should be marked upon the drawings, the patterns, and the special tools pertaining to each piece, and, when convenient, upon the piece itself. Commercially, it is required on time cards and in indexes and pattern lists and cost books, as pertaining to production. Pertaining to sales these names or symbols must appear in illustrated price lists, and in orders by and charges to customers. This our modern method of repairs, by selling duplicate parts, renders imperatively necessary.

The requisites for a good system of names and symbols are: 1st. *Isolation* of each from all others that did, do, or may exist in the same establishment. 2d. *Suggestiveness* of what machine, what part of it, and if possible, the use of said part—conforming, of course, to established conventional names, as far as practicable. 3d. *Brevity*, combined with simplicity. Of the importance of isolation to prevent mistakes and confusion; of suggestiveness to aid the memory; of brevity to save time and trouble, it is hardly necessary to speak.

Regarding the systems now in use in our best shops, this paper will not attempt detailed information. It is understood that the names are more or less scientifically arranged; depending, of course, upon the amount of study and the quality of the brains that have been expended upon them. In cases where symbols are used, supplementary to the names, they usually consist of letters or numbers, or (oftener) a combination of both. Many of them (both names and symbols) fail in symmetry and suggestiveness, because little attention has been paid to the names of the machines themselves, as regards the serial consecutiveness, hinted at in paragraph 2d. The quality of brevity often suffers severely, because the name and symbol must, in most cases, each have the machine name prefixed, to secure their perfect isolation. The latter quality is rarely dispensed with, simply because the manufacturer's pocket would be too directly touched by the expensive resulting mistakes. A perusal of some machinery catalogues which give detailed lists of

parts is very harassing to a systematic mind. They are apt to derive one part name from another, prefixing the latter as an adjective each time, until some such pleasant title as "lower-left-hand-cutting-blade-set-screw lock-nut" is evolved. If there are symbols provided, they consist of some unknown combinations of letters part way down the list, and then change to arbitrary numbers, or perhaps to nothing at all. It will often be noticed also that no particular order appears to be followed in numerical arrangement, similar parts being scattered at random through the list.

The scheme to be described further on has been evolved gradually from the experience gained in managing a growing machine business. This scheme is far from perfect, and is probably inferior to others which have not been made public; but it seems to answer the purpose aimed at, viz., a comprehensive and elastic system which will accommodate itself to an unlimited growth and any variation in quantity or kind of goods manufactured. This the methods we first tried would not do, being too limited in their scope.

It should be here explained that the word "we," as just used, refers to the above-mentioned machine works, with which the writer has long been connected; and the scheme in question will be spoken of as "our symbol system." To further define terms: "machine name" and "machine symbol" refer respectively to the name and symbol of the whole machine, or other article of manufacture, for it will be noticed that the system is applicable to almost any products, except those of a textile or chemical nature. "Piece name" and "piece symbol," in like manner, refer to the separate pieces of which the whole is composed. The terms "detail," "part," and "piece" have so far been used synonymously. It is doubtful which is really the best to establish as a standard, but we have adopted "piece," as best expressing the idea of one piece of material, reduced to the last condition of subdivision. In our practice exceptions are made to this requirement of homogeneousness in such cases as chains, ropes, belts, etc.,—also material glued or welded together,—in short, anything which may (like a man) be called *one piece*, because it is not intended ever to be taken apart. The character for equality (=) will be used to show connection between a name and its symbol. A brief glance at the history of our system shows that at first we (like many others) hit upon the plausible idea of using numbers for machine symbols and letters for piece symbols. The numbers were some-

what "gapped," but not to such an extent as we now should practice. Examples: If four sizes of pumps were symbolled 1, 2, 3, and 4, their barrels might = 1-A, 2-A, etc., and their handles = 1-B, 2-B, etc. If the next product made was a series of lathe dogs, they would probably be symbolled 11, 12, 13, etc. Their frames would = 11-A, 12-A, etc., and their screws = 11-B, etc. This all worked beautifully until the products became so complicated as to contain more than twenty-six pieces! After tampering a little with the Greek alphabet (which seemed calculated to scare our new workmen), and trying to use a mixture of small and capital letters (which looked too near alike), we fell back upon the clumsy device of repeating the alphabet, with letters doubled or tripled.

When we finally abandoned the above plan, several methods were carefully studied. The next most obvious was to use letters for machines and numbers for pieces. This allowed any quantity of the latter, but limited the machines to twenty-six, even with no gaps provided. A certain modification of this method is, perhaps, more in use than any other system. In it letters are used for different sizes or styles of a certain kind of machine, and used over again for some other kind, *ad infinitum*. This answers the purpose, because there are not likely to be more than twenty-six varieties of one machine. It has, however, the fatal objection of requiring the whole machine name prefixed to each symbol, in all cases where the symbol stands alone, and does not happen to be written with the others of the set in tabular form. As the general name of a machine usually consists of at least two words, a complete piece symbol becomes too long for convenience in labelling. Examples: Force pump, K-26; lathe dog, H-2.

Another system consists in using numbers for the machines and numbers for the pieces. This gives isolation and brevity, but no suggestiveness. A serious objection to it is the danger of blurring the numbers together or of transposition in writing or reading them; also in the fact that either number cannot be used alone, as it can in the case of letters and numbers.

A similar system to the above consists in the use of letters for both symbols. It has the same disadvantages, and the additional one of a limitation in the quantity of letters at disposal.

Our system, as finally decided upon, is as follows: *Machine names* and *piece names* are determined by the designer, in general accordance with the principles already pointed out, being, of course,

made as brief and suggestive as possible, with no two machine names alike and no two piece names alike in the same machine. In this nomenclature no positive laws can be followed but those of common-sense and good English. A *machine symbol* consists of a group of *three* arbitrary *letters*—capitals. A *piece symbol* consists of an arbitrary *number*, and follows the machine symbol, connected by a hyphen ; thus FPA-2 might symbolize the force-pump handle before alluded to—smallest size. The machine symbol may be used alone when required, as FPA.

As thus described, these symbols fully possess the qualities of *isolation* and *brevity*. To make them also *suggestive*, some attention must be paid to what letters to use. In practice, we aim to make the first two letters the initials of the general name of the machine, and the last letter one of an alphabetical series which will represent the sizes of the machine. An example of this is shown in the symbol for the smallest sized force-pump, FPA. If there is any chance of a future smaller or intermediate size, gaps should be left in the alphabetical order. This "initial" method cannot always be strictly followed, because of such duplicates as FPA for force-pump and foot-press. The remedy would be to change one initial for one beginning some synonymous adjective ; that is, foot-presses might be symbolized TPA, assuming that it stands for treadle-press. Usually the least important machine should be thus changed. From this it will be seen that, in defining the theory of this scheme, the words "arbitrary letters" were purposely used. The idea is to make the system thoroughly comprehensive. There might be such a number of machines having identical initials that the letters would be almost arbitrary. In practice, the designer can usually succeed in making the symbols sufficiently suggestive.

In considering how many letters to use in a symbol, considerations of brevity advised two ; suggestiveness, three or four. Two letters did not allow of enough permutations nor indicate well enough the kind and size of machine. Three seemed amply sufficient in the first respect, as it provided over 17,000 symbols. If, for any reason in the future, four letters should seem desirable, the addition of another would not materially change the system. If three letters, hyphened to a number of one, two, or three digits, should seem bulky, remember that this symbol can stand by itself anywhere and express positively the identity of the piece. Its comparative brevity is shown by comparing the

second ana third columns of the following table (A). In the different lines an idea is given of the application of the system to a variety of products not usually made in any one shop.

TABLE A.

Col. 1st. Full name of machine and piece.	2d. Our symbol for it.	3d. Symbolic name as often used.	4th. Characters in col. 2	5th. Characters in col. 3	6th. Excess of Col. 5 over 4 ....
6″ × 4′ engine lathe, spindle head.	E L A-4	Engine lathe, A-4	4	13	9
No. 4 power press, frame	P P D-1	Power press, D-1	4	12	8
7″ × 14″ steam engine, crank shaft.	S E G-51	Steam engine, G-51	5	14	9
Buckeye mow'g mch. left axle nut ..	M M D-81	Mowing machine, D-81	5	16	11
No. 3 glass clock, main spring	G C C-105	Glass mantel clock, C-105.	6	20	14
One-hole mouse trap, choker wire ..	M T A-3	Wooden mouse trap, A-3 ..	4	17	13

Table *B* is a specimen of part of a page of our "Symbol Book," in which are recorded any machines which have arrived at such a state of perfection and salability as to be marked "Standard" on our drawings.

TABLE B.

FPL		No. 3 FOOT PRESS.			Weight.		482.
Piece number.	Same as	Piece name.	Material.	Quantity.	Rough weight.	Finished weight.	Aggregate finished weight.
1		Frame.	Cast iron.	1	220	200	200
2		Gib.	"	1	10	9	9
3		Slide bar.	"	1	45	40	40
4		Front leg.	"	2	30	30	60
5		Back leg.	"	1	40	40	40
6		Treadle.	"	1	17	15	15
7		Lever.	"	1	85	80	80
8	FPH-8	Lever weight.	"	4	5	5	20
9		Pitman.	"	1	12	10	10
10	FPH-10	Clamp sleeve.	"	2	3	2¼	4½
21		Lever pin.	Steel.	1	2¼	2	2
26	FPJ-26	Treadle & Pitman bolt.	Iron.	3	½	½	1½

This table almost explains itself. The piece numbers in first column do not have the letters prefixed because the latter stand at the top of the column. "Same as" means that the piece is identical with a piece belonging to some other machine, and can be maufactured with it. If it is common to several machines in a set, the smallest of the set in which it occurs is given. The "quantity" column tells the number of pieces of a kind required. The last "weight" column, added upward, shows total

weight of machine. The piece numbers are "gapped" after each kind of material, and also at the ends of "groups," as described further on. This is to allow for future changes and additional pieces; also that other nearly similar machines, having more pieces, may, in general, have the same piece numbers.

The order in which the pieces are numerically arranged cannot follow positive rules in all cases. In our list of instructions (too long to be here quoted) we direct a classification by *materials*. In each class we group pieces of the same general character, in regard to the prevailing work to be done upon them, and in natural "machine-shop" order; *i. e.*, first planing, then drilling or boring, then turning. We also aim to place the heaviest and most important pieces first. Between each group we "gap" the numbers.

Regarding position in naming pieces, we assume a front to the machine (where the operator is most likely to be placed), and define direction tersely as "forward," "back," "right," "left," "down," "up." The adjectives of position prefixed to piece names are, of course, derived from these words, as "upper," "lower," etc. A perpendicular row of similar pieces, say five, would be rated upper, second, third, fourth, and lower. A number of different sized pieces of similar name may, in like manner, be prefixed smallest, second, third, etc.

Before closing, a brief reference to certain (two) supplementary symbols may not be out of place. One is a small letter after a piece symbol (as FPL-21-a), signifying that the piece is obsolete, the standard, FPL-21, having been altered. After a second alteration, the last obsolete piece would be suffixed "b," and so on. Thus duplicate pieces of old-style machines can be identified and supplied to customers. The other symbol referred to is to indicate the number of the operation in the construction of a piece, and is written thus: FPL-21-1st, FPL-21-2d, etc. Its use is of great value on detail drawings, time-cards, and cost records. It enables any operation (no matter how trivial), on any piece of any machine to be identified by a symbol alone. An *operation* we define as any work which is done by *one person* at *one time*, before passing the piece along and commencing upon another.

DISCUSSION.

MR. STRATTON: I would like to speak in regard to the classification of castings. The system here in use does not extend to

forgings; but in castings it seems fairly to be used, so that castings can be furnished easily. All car castings are classed V, beginning at a small number and running up. It may run from one on up, and, after all the castings have been numbered, then a number of vacancies occur. Each casting has its letter and its number cast on it. In the catalogue engine castings are called class X. Ferry-boat castings are classed F, and numbered from one up. Printed catalogues are furnished to all interested parties, and in those catalogues the piece is described by name and the letter and number are printed on the same line; so that a person, if he is in doubt at all about the casting, he sees a description of it in the catalogue, or if he happens to have an old casting in stock, he can refer to it and find the number on it. The name is not used in ordering, which saves writing, as in the system explained in the paper read. Their forgings are not numbered in any way. When a number of engines are ordered a complete list of the forgings required is written out. The catalogue is written up by the foreman of the shop, showing how many forgings pertaining to the lot of engines have been made. We encounter the same difficulty of a running out of numbers, not having allowed enough for new patterns. It not being necessary, however, to have all the numbers of a car, or car-truck, or engine consecutive, the number which is given to each piece is as distinctive as if they were consecutive. All engine-castings being classed as X, and all car-castings as V, we are not likely to run through the alphabet. If we make, say an occasional crane, we use some other letter. It is not likely that we would have a sufficient variety of work to use up the alphabet.

MR. PARTRIDGE: In certain lines of business there is one trouble which would arise from a system like that Mr. Smith speaks of, which is felt very heavily. A catalogue is published this year with TPA-2 as the standard style, and it goes out to Australia and twenty years from now your Australian customer writes on for TPA-2. In the meantime that has received an extra tail and become TPA-2-a, and his order comes around, and without any hesitation, you send the present standard, and in eighteen or twenty months, as the case may be, your customer sends back word that that won't fit the piece that he is using, made in 1860, or thereabouts, and that he wants something that will fit that. So that in certain trades it will not answer to alter a designation which you have once given to the public. You must be content

when you publish a catalogue for 1889 to make your designations a little longer than they were for 1881.

When it comes to such a thing as getting a set of journal boxes ordered from Constantinople to be shipped by the only sailing vessel that is likely to leave your port in six months, and after it gets there find that you have sent a casting which does not fit those cars, you are in rather an uncomfortable position. A very nice illustration of the necessity for uniformity in the nomenclature occurred some years ago. A railroad car line in Constantinople ordered of John Stephenson an "oil pocket," or set of them, and after going over his card as carefully as he could, the only thing that he could think of which would answer that was the journal-box, and a journal-box was sent. I at this moment forget what casting it was, but the journal-boxes or whatever they were were not wanted, and the cars had to wait until another shipment could be made to Constantinople to fill the bill. I think a sketch was made on the second order to show what was needed. In the manufacture of street cars that want, perhaps, has been felt more than any other requirement of the trade, and has caused infinite trouble to those manufacturers who have done an export business. Even the *Master Car Builders' Dictionary* has not covered the street car, and the curious part of the street car business is that the names which you think ought to stick on the roads and in the shops will not stick. You get a philosophic name for a part, but your workmen will not use it, and your patrons will not order it by that name, and you have to take them as they come. It is very much like the slang of our language, and in speaking of material objects and mechanics men are very prone to use slang in the same way that we find our boys on the street. There is some witticism connected with it, or something that is pungent or *apropos*, or strikes the fancy, and the men seize on it.

Mr. Smith: It is very difficult, indeed, in getting orders from customers, especially for separate pieces, to deal with the names they give. We have found that out, and oftentimes it is necessary to confer with them again. We have not made a specialty of dealing in parts to a large extent, but a customer in ordering should state what catalogue he refers to or what machine it is for. Where the machines are individually numbered, if the man will only tell what machine he wants it for there will be no trouble at all. We can trace right back by these records whether that piece has become obsolete or not.

Mr. Partridge: I may say that I come in contact constantly with catalogues of all kinds, and there are a great many manufacturers who omit to date their catalogues. They seem to have the feeling that they want to keep the catalogue as long as possible, and if they put the date on, people will think it is an old machine they are sending out. That very valuable feature of the catalogue is omitted entirely. I think that more than half of my collection of catalogues, and I have a large one, are undated.

Mr. Porter: This matter of nomenclature is one that is very interesting, without going so far as Mr. Smith has gone in his very elaborate and admirable system, and one, also, so difficult to keep up and requiring somebody behind it all the time. I have been reminded of the difficulty of translating our technical terms into any other language. It is utterly impossible to get a translator to render into any European language with any certainty the terms employed in mechanics. The funniest blunders are made of that kind. I was told of a case where "hanger" was translated into German by the term signifying "chandelier." I am told that on the average that is about as near as translators or technical dictionaries get.

Mr. Partridge: When I want a translation made I give it to a man who has not any knowledge of machinery at all, and tell him to translate that literally right through, and I get the most laughable piece of composition in English, or French, if it is French, that you ever saw; but you can follow it better than if a man takes a mechanical dictionary and tries to turn it into mechanical German or French. Try that and you will be able to get at your results without the ordinary mistakes.

Mr. Smith: I would say that these machines, whenever they are changed, are marked "Style A," "Style B," etc., and although we have not yet gone into an elaborate system of catalogues for these duplicate parts, we are preparing for them ahead. When you extend it to details the only proper system is to give a picture of each detail, and require that your customer give the date of the catalogue and style of the machine. If he won't do that; if he lives in Australia and after twenty years telegraphs for FPA-2, I would not send it to him. You cannot humor customers in all their vagaries. They have got to have some sort of definiteness in their orders. As to translation, you need not translate at all. It is a system of hieroglyphics.

Mr. Partridge: The hand-pumpmakers have a plan—it was

commenced by Douglas when he first began, I think—who made a picture of every style, and as he put it in the catalogue called it a Figure. It was not a very philosophic name, but all hand-pumps now are Figure 1, 2, 3, 4, and that systematizes the whole thing. When Figure 1 begins to be obsolete or is changed a little, the changed pump was engraved, put in the catalogue and called Figure 7, and when that was changed again it was called 264, and the numbers were promiscuously filled up. In some shops even hundreds are used to separate classes, getting one hundred in a class, or more, sometimes two hundred. The number enables them accurately to designate any pump or any alteration in any pump which they have made and without any great complexity of system. That has been a very successful thing in connection with foreign countries and in foreign trade. It is one that is understood. Their injunction is repeated over and over again,—send us figure and number; and if they do that they cannot make any mistakes.

Mr. Smith: Ours is essentially the same. We use Press-number 4 and Style H. If it is Press-number 4, Style F, of course it is different. I think in the names of pumps there is some confusion. I think they have the pitcher-top pump and pitcher-spout pump. They have a Number 1 pitcher-top and Number 2, etc., and then they will have a Number 1 pitcher-spout and Number 2 pitcher-spout, etc.

Mr. Partridge: The word Figure and two numbers following it, will order any pump in the catalogue.

The President: I would mention that those of the members who are interested in this system of indexing and in the system of nomenclature referred to by Mr. Smith in his paper, will probably find something more to interest them in a system in use at the Frankford Arsenal. Major Metcalf, who is stationed there, has originated the system, which is something similar in its general principles to that described by Mr. Smith. In detail it is different and applied to a different kind of work, but it is ingeniously devised and works very satisfactorily, indeed, and it is simple and effective. I have no doubt that Major Metcalf will be glad to give an explanation of his system to any of the members who apply to him on the subject and to furnish them with specimens of his cards.

XLVIII.

METHOD OF ARRANGING AND INDEXING DRAWINGS AND PATTERNS.

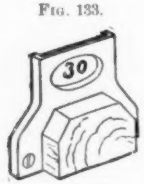
BY ALBERT F. HALL, BOSTON, MASS.

Any time which the engineer devotes to perfecting the arrangement and indexing of his drawings will be amply repaid by the time and trouble it will eventually save him; and if the writer of this article is able to assist, even to a slight degree, his professional associates in attaining the desirable object, his efforts will be fully rewarded.

In what follows, it will be the aim to point out how to proceed when indexing a new set of drawings, and also how to remodel a collection, arranged in the ordinary manner, to conform to what the writer considers a more convenient system.

For the sake of uniformity and ease in handling, the writer has made all of his drawings on either full, half, or quarter sheets

Fig. 133.



of Whatman's "double elephant," and has arranged them in drawers of corresponding sizes, and about two inches in depth. Each drawer is provided with a peculiar handle, having a pocket for a removable label, a sketch of which is appended. (See Fig. 133.) The drawings are all numbered on each of the four corners, the numbers on the left-hand end being upside down, so that they may easily be read should the drawing have been turned around in the drawer. Every drawer is labelled with a number corresponding to the drawing bearing the lowest number in that drawer. To avoid confusion, no two drawings have the same number simply, letters being used in connection with the numbers, as we shall see later. The index and references always tell on which of the three sizes the drawing is made, and, consequently, in what set of drawers it will be found. To illustrate, let us take a drawing of an engine, 9 × 16, made on a full sheet

and numbered 45; the index card referring to this engine will have,—"*drawing full sheet* 45," which tells at once the set of drawers, and the position in those drawers, where it must be sought. On the main drawing of the engine together, or upon a sheet referred to on that drawing, will be found references to the various details used in its construction, thus: Steam cylinder, ½ sheet 45—A; steam piston, ½ sheet 45—B; cross-head, ¼ sheet 45—C, etc. It may, of course, happen that parts which have been used for other engines are also to be used for this, then we shall have something like the following: Connecting-rod, ½ sheet 36—B; stuffing-box, ¼ sheet 27—M, etc. Each one of these details will always have a reference to the machine for which it was first made. Where there are many parts of a kind and size, but varying in design or arrangement, it is well to group them and give them all the same number, but different letters;

Fig. 134.

12" Steam Pistons.

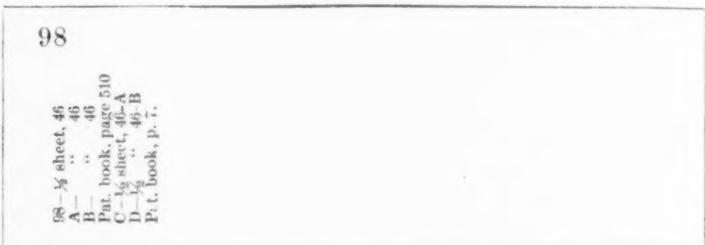
½ sheet, 46
" " 46–A
" " 46–B

by so doing we can easily compare them and see at a glance what has been done, and whether any of them can be utilized in a machine in process of construction. Suppose, for instance, the drawing to be of a 12" steam piston, and this drawing to be ½ sheet 46; now all other 12" steam pistons would have this same number, with various letters, and would, preferably, be on sheets of uniform size. The index card for such would be represented by Fig. 134. The card not only tells the drawings but the letters already used, and thus prevents repeating a letter. When all of the letters of the alphabet have been used, a new number may be taken and another group be formed, though the letters may be doubled. Such groups are very useful, as they are soon learned, and reference to the index often becomes unnecessary. Whenever a pattern is to be changed, a new drawing should be made, and the change carefully indicated, reference being given to the original drawing, which drawing should also have a note stating the changes that have been made, and where these changes

are to be found. Even if such changes are to be permanent, the original drawing should not be erased, as it is oftentimes necessary and important to preserve the record, and it is better to make a new drawing.

All drawings should be indexed on cards of uniform size, about the size of a postal card, and arranged by titles in alphabetical order, cards of different letters being separated by thin strips of pine wood or by a zinc plate of the size of the card, this plate having one edge bent so as to form a lip, which projects sufficiently for writing the divisions; the plates are sold by the Readers' and Writers' Economy Company. Where there is a group of details, such as steam pistons, a slip with the title, steam pistons, may be inserted, and the sizes arranged in numerical order in this division. On many of these cards little descriptions are written, and sketches sometimes added giving

FIG. 135.



the principal parts, so as to avoid, oftentimes, reference to the drawing.

The tracings are kept in a duplicate set of drawers for the use of the workmen.

Having now described the manner of arranging and indexing the drawings, it remains to show the method adopted for keeping a record of the patterns.

Every pattern has a number, which is given on the drawing, the main piece having the simple number, and all loose pieces and parts belonging to this bearing the same number, with the addition of various letters. Thus, the patterns for the 12″ steam piston would be numbered somewhat as follows: Head pattern 98, follower pattern 98—A, ring pattern 98—B, etc. Card indexes are arranged, giving the various pattern numbers, and referring to a book wherein a description and other particulars of these patterns are given. These cards are made as shown in

Fig. 3, and are arranged in numerical order in a drawer, each group of one hundred being separated by a wooden strip or zinc plate, as described above. These cards also show the letters that have been used in connection with any number. Should a piece be misplaced in the foundry, it can be traced, by the number which is stamped upon it, to the index, and by that to the draw-

FORM 1.

Pattern, ...	98
Drawing, ...	½ sheet, 46
Date when Finished, ...	July 28th, 1881
Material, ...	pine
Hours Labor, ...	

ing and pattern book. In ordering castings, the parts which are wanted can be designated by the number and letter. In addition to the above, each patternmaker receives a printed slip, a sample of which is appended, and designated Form 1, to be filled out and handed by the foreman to the draughting-room on the completion of the pattern, to be recorded in the book alluded to, a

FORM 2.

No. of pattern.	Drawing.	Date when finished.	Material.	Cost of material.		Hours labor.	Total cost.	Description and for what used.
				Per foot.	Total.			Indicate where change is permanent.
98	½ sht. 46	July 28, 1881.	pine.					Head of 12″ steam piston.
" A	" 46	" 29, 1881.	pine.					Follower for 12″ steam piston.
" B	" 46	" 30, 1881.	pine.					Rings for 12″ steam piston.

leaf from which accompanies this article, and is designated Form 2. An experience of about nine years has shown the practical value of this system to the workmen and all others who have occasion to consult the drawings; it is much superior to the ordinary method of cataloguing in a book, as it permits of a systematic arrangement, impossible in a book unless no more drawings are to be added to the collection. The great advantages of

uniformity in the sizes should not be overlooked, for, where the drawings vary in size, the smaller sheets are liable to slip under the larger and thus escape notice. Some have observed that their drawings vary so much in character that they cannot be classified, yet all must have titles, and can easily be alphabetically arranged by these titles. Again, others say they have so few drawings that it will not pay; to such the answer is,—who knows how rapidly the drawings will accumulate and reach a state of chaotic confusion? The writer was fortunate enough to commence with this system, and time has shown him its incalculable value, his collection having grown to about three thousand drawings.

It will now be shown how a collection consisting of over 3000 drawings was remodelled from an imperfect system.

A book was printed containing, on the left hand of each page,

FORM 3.

Old Numbers.	New Numbers.	Remarks.
1.	½ sheet, 78	
2.	¼ sheet, 89 B	
3.	Full sheet, 13	
4.	½ sheet, 74 D	
5.	¼ sheet, 83	
6.		
7.		
8.		
9.		
10.		

two columns, one with the heading "Old Numbers," and the other with the heading "New Numbers," the remaining space being left for "Remarks." The column "Old Numbers" was printed with numbers in numerical order from one upwards, these numbers being arranged in groups of five for ease in finding. (See Form 3.) The drawings were then taken as they came along, renumbered, lettered, and arranged in sizes to conform, as far as possible, to full, half, and quarter sheets of "double elephant." As fast as the drawings were renumbered and lettered, their new numbers were entered in the column "New Numbers," opposite their corresponding old number, to be found in the column "Old Numbers." The arranging, grouping, and indexing were carried on as the drawings were renumbered, in order not to mix them up, thus avoiding confusion, and enabling the draughtsman and others to use the index as the work progressed. Since this method has been inaugurated, all concerned with it are very much pleased,

and wonder how they managed so long without it, saving, as it does, all confusion and trouble.

DISCUSSION.

MR. VOGT: I would like to ask how those cards are kept. I do not quite comprehend.

MR. WOODBURY: They are kept in drawers, rather shallow drawers, with longitudinal partitions, and are separated by a zinc plate or a wooden plate. For instance, there may be steam-pistons there, or steam-engines, or steam-pumps, or any part. There is a similar arrangement for cataloguing all the patterns, but instead of being upon white paper they are on brown paper, and of the size of that piece of wood.

MR. PORTER: We have adopted in our practice a system of cataloguing and arranging drawings which in some of its features is similar somewhat to the one presented in the paper of Mr. Hall. It differs in this,—it has so few features. It is extremely simple and is found to work in a very admirable manner, and I thought when I first learned this morning that the paper of Mr. Hall's would be read, that I would, in the discussion of that paper, present our system verbally; but I have changed my mind and have concluded that I will present at the next meeting a paper on the system. It is very simple, but works so well that I think it is worthy of being presented in the form of a paper.

PROFESSOR ROBINSON: I think there is considerable difficulty from want of a due distinction of drawings. I would divide drawings into three classes that we have to contend with in getting our machinery. First, have the designing drawings, because those are the drawings upon which we work to solve the problem of the design of the particular machine in hand. We assign the sizes of the parts, and look out for interferences, etc., and get the parts all shaped and proportioned. We call that the designing drawing. Then, after this, we have drawings which might be styled detail drawings. These drawings may be made out from the designing drawings and only one piece to a drawing, if you prefer, but we bring in enough projections or views to enable the patternmaker or machinist to understand it fully, without asking a question of anybody. Then, these drawings are all that are necessary by which to make the machine. We make this designing drawing simply to solve the problem of design; and the detail drawings to go to the artisans, and the pieces are made. We have got the machine

made and the next thing is to put it together. If this should be attempted without any reference to the man who designed the machine then certain other, or a third class of, drawings are necessary. These drawings could be called finished or completed drawings, which would represent the thing complete, and the elaboration of these drawings may be very considerable, or very little. It may be finished up with tinting and shading so as to appear as good as any steel engraving. Some drawings of this sort cost two or three hundred dollars a piece. In some of those instances they understood that this would be necessary to sell the machine. That is one feature of the value of these costly drawings. It may be necessary in order to sell the machine to have these complete drawings. This drawing is also valuable to enable the master mechanic, who may not have had any hand in the designing of the machine, to know how it is to go together. He thus knows where the parts go. This drawing may be a finished drawing and tinted up in perspective, or it may be a drawing in sections, longitudinal, transverse, or horizontal, etc., or it may be a drawing which is compound, consisting of projection and sectional drawings, both combined in one. We would have these three distinct classes of drawings, and it seems to me that in a perfect system the three would be necessary.

Now the designing drawing may be of any size that is convenient. It may be spread out on two or three tables put together. It should be made so that the parts are in correct proportion, however. The detail drawings may all be made to one size of sheet. I have succeeded myself, without any difficulty at all, in putting all the parts of a machine in detail, some of them being six or eight feet in dimensions, upon sheets fourteen by sixteen inches, and no sheet was introduced larger than that, and none smaller than that, some thirty or forty sheets representing the thirty or forty pieces in the machine, all represented on this size of sheet. On this sheet the figures were given for the dimensions. Nobody nowadays would measure a drawing to get the size they want. The drawing should be large enough to leave room for the numerals read without any question.

When you come to use the detail drawings you want them in the best shape for the workmen to use. That is an important point. If a man has to run from his lathe to the wall where the drawing is hung up, he spends as much time as it would take to finish the machine in running back and forth for these figures.

This time can only be rendered valuable by having the drawing in some smaller size than 2 × 3 or 3 × 4 feet. If it could be brought down to 10 × 12, or 12 × 14, or 14 × 16 inches, it could be pasted on a piece of board or card and placed on his lathe, or it could be held up in front of the lathe in a little vise. I think that a very good thing. Probably the most convenient size for it would be 10 × 12 inches, but I think that, as a general thing, it should be larger than that to suit the wish of most machinists. It can be up to 14 to 16 inches, and be held in that way. If handled by the machinists the drawings are in danger of being smutted and covered with dirt, so that there is uncertainty about what the figures are. You must either have them so that they can be clean or in sufficient numbers to keep the machinist supplied. One of the most convenient ways of getting around this difficulty is by the blue print process. The detail drawings may be pencilled first on paper and then inked on the tracing cloth, which may serve as negatives to make the blue prints from. Any number can be made, and then they can be mounted on paste-board and each machinist can have one, and after these drawings have served the purpose they can be laid aside. I think that they could get along in our manufacturing establishments by filing the drawings in this way. The drawings are filed away in a portfolio of some sort, and there may be extra sets of these laid away on file—that is, extra copies. Then, after the parts of the machine are made, these drawings are of no further use, and you only have to refer to the finished drawings, if it is necessary to do that in putting the machine together. Every manufacturing establishment, I think, has lost money by not having a standard regulation size of all the drawings. It may be ten or twelve inches or some other size. I have seen this done in some establishments—not with blue prints, but with hand copies, but now blue prints are cheaper. I think now if all the manufacturing establishments of the country would act on this basis they will find this matter of drawings much improved and that machine work would be done much cheaper in consequence of it.

Mr. Smith: I would say that it seems unnecessary to have three distinct classes of drawings. In our practice we have two. The *designing* drawing has been necessary to get the design. Then make the details—small scale detail drawings—and afterwards this designing drawing is completed and made the *erecting* drawing, as you might term it. Of course it cannot

be on as large a scale as you might wish for some graphic work; but in almost all cases it can easily be enlarged on a temporary scale, and rough drawings made to find out interferences, etc., so that it is not necessary to have more than two sets of drawings.

Referring to Mr. Hall's paper, I would say that we use six sizes of paper, and they are all based on a sheet of 24 × 36, and cut from that without waste. Each sized paper is known by a letter, so that we do not have to say "half-sheet" and "quarter-sheet." They are lettered in *natural* order, with A for the smallest. A is $4\frac{1}{2}$ × 6 inches; B is 6 × 9; C is 9 × 12; D is 12 × 18; E is 18 × 24; F, the largest, is 24 × 36. This sheet I have in my hand is B size. They are labelled and numbered in one corner *diagonally*, so that label will read aright with either *end* or *side* of sheet at top, as may suit the particular drawing in hand. We have sets of drawers (in a very large fire-proof safe) for each general class of drawings. Each set has compartments for all the sizes of paper. Every drawing is indexed numerically, and then there is a classified index or "ledger" made from that, and there you can find what you want very readily.

Mr. Bond: I would say that in the drawing-room at the Pratt and Whitney Works a form has been adopted similar to what Mr. Hall suggests, having double elephant size and dividing that, having the different sizes marked A, B, and C. The largest is A, the next size B, and the next C.

Mr. Vogt: The sizes of sheet used by the Pennsylvania Railroad are only two. They are called whole and half sheet. The whole sheet is cut out of a roll of paper 58 inches broad. We use altogether this German paper and cut it out in a piece 35 inches. Now a half sheet is 29 × 35 and a whole sheet is 35 × 58, and these two sizes are used exclusively for all kinds of drawings. The designing drawings we call general arrangement. It is always drawn on full-size sheets. A link motion and a set of rods, or something like that, will require a large sheet, while some engines will require only a small sheet. The drawings are never detailed out very extensively. For instance, the details of a link motion give eccentric, rods, link, hanger, and everything of that kind on one sheet, and all those parts are assembled together properly relating to each other. If any details are made out afterwards they are traced off from this sheet. Tracings are sent out, or rather blue prints now, and frequently when

they arrive in the shop a man in Mr. Stratton's office makes a sketch of the various details—not necessarily to a scale at all—traces it off and makes blue prints. These are pasted on cards or boards and sent down to the shop. The drawings are distributed around in drawers, according to the size of the paper, and they are so arranged that we have the back end of the locomotive by itself, the front end by itself, and so on. The indexing of the drawings is done in a very simple way. We have a large index book with an index along the side: A certain number of pages are set aside for each letter and the drawings are indexed once, or twice, some of them. Every drawing is numbered consecutively, no matter whether for cars or for locomotives. No matter what it is, the last drawing has the last number. We found it impossible to set aside a certain class of numbers. We found it to answer tolerably well—not as well as it ought to do, perhaps. I have no doubt that Mr. Hall's system is a very good one. Of course I have not tried it. It seems very easily handled. We have three thousand drawings, and if we detailed them out we should probably have ten thousand. Ours are not very much detailed out.

Mr. Woodbury: He has over three thousand and he has considerable detail to his drawings.

Mr. Vogt: We seldom employ over a three-inch scale to the foot. If we should detail out everything we should get an enormous number of drawings.

Note.—Since presenting the above, the letters A, B, and C have been substituted for full, half, and quarter sheets. There is really very little difference between this arrangement of the drawings and those described by Mr. Porter at the Annual Meeting of 1881. It is believed that Mr. Porter and others would soon see the value of grouping a *few* of the principal parts in *our* business. No difficulty or loss of time has been experienced in the shop in the use of this system.

Albert F. Hall.

PROCEEDINGS

OF THE

NEW YORK MEETING, 1881.

XLIX.

PROCEEDINGS

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

SECOND ANNUAL MEETING,

NOVEMBER 3D–4TH, 1881.

THE meeting was called to order in the Turf Club Theatre, New York City, at 3 P.M., on Thursday, November 3d, by the President.

The following members were in attendance:

F. B. Allen,	Brooklyn, N. Y.
Thomas R. Almond,	Brooklyn, N. Y.
William Atwood,	Brooklyn, N. Y.
Jackson Bailey,	New York City.
Stephen W. Baldwin,	New York City.
James C. Bayles,	New York City.
James Brady,	Brooklyn, N. Y.
A. C. Christensen,	Brooklyn, N. Y.
Thomas L. Churchill,	Boston, Mass.
Alfred Colin,	New York City.
A. W. Colwell,	New York City.
George N. Comly,	Edgemoor, Del.
John Cotter,	Norwalk, Conn.
D. P. Davis,	New York City.
Joseph P. Davis,	New York City.
Charles P. Deane,	Springfield, Mass.
James E. Denton,	Hoboken, N. J.
Albert H. Emery,	New York City.
S. H. Finch,	New York City.
Alexander Gordon,	Hamilton, Ohio.
Albert F. Hall,	New York City.
F. F. Hemenway,	New York City.
William Hewitt,	Trenton, N. J.
D. S. Hines,	Brooklyn, N. Y.
Frederick R. Hutton,	New York City.
F. W. Jenkins,	Brooklyn, N. Y.
L. G. Laureau,	New York City.
E. D. Leavitt, Jr.,	Cambridgeport, Mass.
W. Barnet Le Van,	Philadelphia, Pa.
Lewis F. Lyne,	New York City.
Samuel McElroy,	Brooklyn, N. Y.
David N. Melvin,	Staten Island, N. Y.
Alexander Miller,	New York City.

Horace B. Miller,	New York City.
Lycurgus B. Moore,	New York City.
Carleton W. Nason,	New York City.
C. C. Newton,	Philadelphia, Pa.
W. E. Partridge,	New York City.
Dwight E. Pierce,	Bethlehem, Pa.
Charles T. Porter,	Philadelphia.
H. F. J. Porter,	Trenton, N. J.
Charles W. Pusey,	Wilmington, Del.
Thomas Whiteside Rae,	New York City.
S. W. Robinson,	Columbus, Ohio.
John B. Root,	Greenpoint, N. Y.
William J. Root,	Brooklyn, N. Y.
Joshua Rose,	New York City.
Horace See,	Philadelphia, Pa.
Oberlin Smith,	Bridgeton, N. J.
H. F. Snyder,	Williamsport, Pa.
Charles Sperry,	Westbrook, Conn.
Albert Stearns,	Brooklyn, N. Y.
Allan Stirling,	Drifton, Pa.
John E. Sweet,	Syracuse, N. Y.
R. H. Thurston,	Hoboken, N. J.
W. P. Trowbridge,	New York City.
Egbert Watson,	New York City.
Samuel S. Webber,	New York City.
William H. Weightman,	New York City.
Frederick M. Wheeler,	New York City.
Jerome Wheelock,	Worcester, Mass.
S. B. Whiting,	Pottsville, Pa.
William H. Wiley,	New York City.
Alfred R. Wolff,	New York City.
C. J. H. Woodbury,	Boston, Mass.

The President: In opening the Annual Meeting for the year 1881–82, I have simply to repeat the statement which I gave you at the last meeting of the progress of the Society and of its flourishing condition. The membership has increased considerably. We have now on our list two hundred and ninety-eight names, and a considerable number of applications for membership are before the Council, several of which have been passed already. The number of members will be considerably in excess of three hundred after these applications have been passed upon. The character of candidates seeking to enter the Society, as you will see, has been carefully watched by the Council. Every application has been scrutinized, and in some cases has been referred back to the proposers with the request that more information be furnished; sometimes for complete modification, and on rare occasions for complete rejection.

The prospects of the Society, so far as I can see, are of the very best. Of the character of our papers you can judge best yourselves. The number has not been great; the quality has, we think, been pretty fair; in some cases remarkably excellent; and the indications are that members are preparing to take more trouble to present to us papers on interesting and important subjects. I think you will find that the papers presented at this meeting embody as valuable matter as any presented heretofore.

The Secretary reports that members are exhibiting a considerable amount of interest in the work of the Society, and the Secretary's work itself is going on now pretty smoothly. There have been many delays in the printing of *Transactions* and a good many hitches have occurred. Of course these must be expected in the beginning of a society's work. But these difficulties will be gradually overcome.

The *Transactions* of the Society are printed and on the way from Philadelphia. They will be circulated among members and distributed by mail to those who are not here, probably tomorrow.

The Secretary has been directed by the Council to have a diploma prepared for issuing to members. That diploma has been designed and is now under way. The printing is going on, and it will be ready in the course of two or three weeks. The original, from which that diploma is made, is lying upon the table at the rear of the hall, and gentlemen interested in the matter can look at it there. They will find also the seal of the Society which it is proposed to adopt upon the incorporation of the Society. A good many have inquired how soon incorporation might be effected, and in some cases gentlemen have objected to joining the Society on the ground that we were not yet incorporated. That matter is now under way, and in the course of a very few days the Society will be an incorporated Society. That will render it necessary to make some slight changes in its methods of doing business, and that will be attended to at the proper time by the Council.

The Secretary has also been directed to prepare tickets to be carried in the pockets of members like those used by the members of the American Society of Civil Engineers, and by the members of foreign societies, and from among several specimens the Secretary has selected the neatest design. The tickets are now ready and will be given to members by the Secretary on their

personal application. He will require their signatures on receipt of them.

It was suggested to the Council, and finally directed by them, that badges be prepared by the Secretary to be used by members at meetings where they would be likely to invite many strangers. Those badges are in preparation, and the design approved by the Council is in the hands of the Secretary, if I remember right, and will be shown you very soon.

It is necessary, or, at least it is advisable, to determine at the present meeting where the next meeting of the Society shall be held. Applications have been made to the Council from members in Boston, asking that the next meeting be assigned, for the spring, at Boston, and we have a very kind invitation from the President of the Massachusetts Institute of Technology to the members to make that Institute their home and hold our meetings there should the meeting in the spring take place at Boston. That matter was discussed, you will remember, at Hartford, and it has been since suggested that Philadelphia was an even more desirable place than Boston. This is a matter which the Council should decide now, and they will be very much obliged to members who will express their opinions to members of the Council in regard to the relative advantages of these and other places. That will probably be decided before we adjourn. If we can once establish as a precedent this determination of the time and place of a meeting before the adjournment of the next preceding meeting, it will save a good deal of delay, probably, in the issuing of the announcements of place and time. A considerable amount of feeling has been manifested from the fact that announcements of place and time have not been made promptly. Those announcements have been delayed usually because the matters requiring determination could not be settled until the action of committees had been taken, which action was delayed, often, until a quite late period; and partly from the fact that the place of meeting was not determined until a considerable time after the preceding meeting. But if it can be determined at this meeting where the next is to be held the call will be issued very much earlier. The members will notice that two calls are issued. The President's call is the official call of the meeting, designating the city or other place at which it is to be held, and the date. The hall in which the meeting is to be held cannot usually be determined until a later date, and the Secretary's

issuance of a programme is generally deferred until a later period when the hall has been secured. So that the President's call should be issued immediately after the adjournment, and the Secretary's programme should follow at as early a date as possible.

The first business in order to-day is the reception, opening, and counting of ballots for officers for the coming year.

The nomination of these officers is confided to a nominating committee, which the President was directed to appoint at the last meeting. That committee was appointed, consisting of Professor Sweet, Chairman, Mr. Delamater, Mr. Merrick, Mr. Kent, and Mr. Wilcox. Their meeting occurred on the 1st of October, and nominations were made, which were sent out under the rule, and the ballots are now to be counted. What will you do about these ballots, gentlemen?

Mr. Hutton: I move that tellers be appointed, Mr. Chairman.

It was so agreed, and the chair appointed as tellers, Messrs. Hewitt, Wolff, and Weightman.

The Secretary then presented his report.

Secretary's Annual Report to Council.

Mr. President and Gentlemen: The record of the year is most encouraging. Our membership has increased from 189 to 297 (including those whose election will most probably be declared to-day), and eight more applications are ready for your consideration. The Society is in correspondence, and exchanges publications, with the Institutes of Civil and of Mechanical Engineers of Great Britain; the Institute of Mining Engineers of Scotland; the American Society of Civil, and the Institute of Mining Engineers; the United States Naval Institute; the London Building and Engineering News; the Industrial Record, the "Cincinnati Artisan," etc.

Our arrangements for publication are at last permanently organized, and the delay that has justly been complained of during the past year will not again happen.

A new catalogue of members will appear as soon as certain amendments to the Rules, now under discussion, are disposed of.

A sample of the Diploma prepared by your orders is ready for exhibition. Its cost, on parchment paper (which is recommended by the engraver), will be 25 cents each.

Steps have been taken to incorporate the Society under the laws of the State of New York, and it may be regarded as an

accomplished fact. The Society is much indebted to Mr. Bogart, Secretary of the American Society of Civil Engineers, for advice in this matter, which has saved it considerable expense for legal service. The badges ordered by the Council will cost $3.58 each. Specimens can be seen by applying to the Secretary, who is prepared to supply all members desiring them. The Cards of Introduction are ready for distribution, and will be issued immediately.

For the better carrying on of the duties of my post, I venture to urge upon the Council the propriety of making an allowance for office rent during the coming year to the Secretary. Our records and correspondence are increasing so rapidly that additional accommodation is indispensable, and the present quarters must be abandoned before long. Until such a step is taken it will be impolitic to collect a library, and valuable chances to do this are lost every little while for this reason. In conclusion, it should be said that the Joint Committee of Invitation to Foreign Engineers, whose latest report is hereto appended, is still sitting.

MINUTES OF A MEETING

Held June 30th, 1881, at the house of the American Society of Civil Engineers, 127 East Twenty-third Street, New York, of the Joint Committee of the National Engineering Societies appointed to consider the subject of an Invitation to Foreign Engineers to visit this Country.

JOINT COMMITTEE.

CIVIL ENGINEERS.	MINING ENGINEERS.	MECHANICAL ENGINEERS.
J. B. EADS,	WM. METCALF,	R. H. THURSTON,
O. CHANUTE,	E. B. COXE,	J. A. BURDEN,
C. MACDONALD,	R. W. RAYMOND,	W. H. SHOCK,
CHARLES PAINE,	W. P. SHINN,	WM. SELLERS,
J. P. DAVIS,	A. S. HEWITT,	T. N. ELY,
T. C. CLARKE,	E. D. LEAVITT, Jr.,	A. L. HOLLEY,
J. BOGART,	T. M. DROWN,	T. W. RAE.

"In response to a call issued by the secretaries of the three societies the following members were present: Messrs Macdonald, Davis, Bogart, Shinn, Leavitt, Thurston, Holley and Drown. Mr. Macdonald was called to the chair and Mr. Drown appointed Secretary.

"Mr. Holley said that the idea of an invitation to foreign engineers had its origin in conversations which he and others had had with some English engineers, who had expressed a desire to make a visit to this country if they could do so under favorable circumstances for seeing objects of engineering and metallurgical interest.

"The object of the meeting as defined in the circular was 'to determine the advisability of extending an invitation, and if approved, to consider in a

general way the number and method of invitations, the extent of the excursions, and the ways and means.' The result of the discussion of these points was a hearty approval of the invitation, and the suggestion of the following general plan to carry it into successful effect.

"It was thought that the number of invitations could be fixed at 150, and that the management of the Engineering Societies abroad should be requested to issue 125 of these invitations. On the arrival of the party at New York, there might be appointed from the three societies, a committee of fifty members to accompany the visitors on the excursions. At the different centres there could be appointed with advantage a local committee of members to join the party for the excursions in the immediate vicinity. It was deemed best that the party should in travelling not exceed 250 persons. A general outline for the excursion was suggested as follows: Boston, returning to New York, thence to Albany, Lake Champlain, Niagara Falls, Cleveland, Chicago (Lake Superior?), St. Louis, Louisville, Cincinnati, Pittsburgh, Altoona, Harrisburg, Washington, Baltimore, Philadelphia, and the Anthracite Coal Regions. This trip would require about a month.

"As to ways and means: Money would be required by an invitation committee for general expenses, and it would further be necessary for local committees in the different cities to raise money for incidental expenses of entertainment. It was, however, to be distinctly understood that the visiting engineers pay their own travelling and hotel charges. These could be made comparatively light by securing reduced rates. The invitations would state the probable expense of the round trip from New York.

"In view of the fact that Messrs. Macdonald and Holley were on the point of leaving for Europe, a resolution was passed authorizing them to make use of the above suggestions, which met with the approval of all present at this meeting, in consulting informally with foreign engineers on the subject. The date of the excursion was provisionally fixed for September, 1882.

"The meeting adjourned subject to the call of the secretary.

"T. M. DROWN,
Secretary.

"EASTON, PA., July 1st, 1881."

Respectfully submitted,
THOMAS WHITESIDE RAE,
Secretary.

THE PRESIDENT: The Secretary's is a report properly to the Council, and it has been referred to the meeting by the Council for the information of members.

The incorporation of the Society is an important matter, and I trust the members will understand precisely what difference is made in the status of the Society by its incorporation. Until incorporated we are practically irresponsible as a body. If debts are incurred by the action of the Society, members are individually liable for them. After incorporation, however, if debts are incurred, the Society is liable, and its members are not. Mr. Rae will read the act of incorporation, in order that you may see precisely what is the status of the Society.

THE SECRETARY: The only portion of this act of incorporation which our rules are contrary to, in any way, is the third section, and that conflicts with Article XXVI of our rules. As we are about to become an incorporated body, our rules should certainly be in accordance with the requirements of the statute. The statute reads:

"The Society so incorporated may annually elect its members, its trustees, directors, or managers, at such time and place and in such manner as may be specified in its by-laws, who shall have all control and management of the affairs and funds of said Society, the majority of whom shall be a quorum for the transaction of business."

Article XXVI of our rules says:

"Five members of the Council shall constitute a quorum."

Our Council consists of eighteen; consequently this Article XXVI is contrary to the statute. Before a new catalogue is published it would be well that this be amended, so that it may come within the terms of the statute. That is the only particular in which our rules are at variance with the act.

THE PRESIDENT: That amendment must take place under the rules.

The election of officers, which now occurs, gives us a new list of officers, which list goes into our catalogue only on the adjournment of the present meeting. It has been suggested that that was, perhaps, not the best arrangement that might be made, and the matter to be considered by the members is, whether it is advisable to make a change in that rule, making the term of office of officers elected at the annual meeting commence at some period in that annual meeting, instead of at its adjournment. The idea which had occurred to one or two gentlemen was, that the annual meeting might adjourn in the midst of its session, calling an adjourned meeting subsequent to it to meet the following half day or day, as the case might be, at which the newly elected officers could take their seats. That would give an opportunity for the formal retirement of officers going out and the formal installation of officers elected. In the American Institute of Mining Engineers, the retiring presiding officer leaves his seat, introducing the incoming presiding officer at the commencement of each annual session. That is a matter for the Society to consider, and after consideration I presume the Society will take action.

MR. RAE: I give notice that the following amendment to the rules will be presented:

"*Resolved*, That Article XXVI of the rules be amended so as to erase the words: 'Five members of the Council shall constitute a quorum, but;' and allow Article XXVI to commence with the words: 'The Council may appoint, etc.'"

THE PRESIDENT: That change can be made at the next annual meeting under the rule.

MR. HUTTON: Do I understand that the change will only be made at the next annual meeting?

THE PRESIDENT: Under the rule.

MR. HUTTON: Would it not be simpler for us to make a motion to suspend that rule by unanimous consent, so that we can take action upon it immediately and not postpone it a year? I would make such a motion if it be in order, that our rules be suspended and that this action be taken at once.

MR. C. T. PORTER: I submit that there is no occasion for immediate action, as the statute takes the place of our rule. So far as the rule is in conflict with the statute it is nugatory. After we are incorporated five members will not constitute a quorum, even though we should try to make them do so.

THE PRESIDENT: The gentleman is perfectly correct. That would seem to cover the case.

MR. BAYLES: There was an amendment to the rules offered at the last meeting, which I presume is in order for consideration at this time.

THE PRESIDENT: Mr. Bayles at the last meeting proposed an amendment to the rules, which properly comes up for discussion at this meeting.

The Secretary then read the following resolution proposed by Mr. Bayles at the last meeting:

"*Resolved*, That the rules of the American Society of Mechanical Engineers be amended as follows:

"Article XI. to read:

"The Society shall claim no exclusive copyright in papers read at its meetings, or in reports of discussions thereon, except in the matter of official publication with the Society's imprint, as its transactions. The Secretary shall have sole possession of papers between the time of their acceptance by the Council and their reading, together with the drawings illustrating the same; and at the time of said reading he shall have printed copies for distribution to members present, and shall give the same to representatives of such newspapers as desire them for unofficial publication, in whole or in part. Copies of the drawings shall at the same time be furnished to journals which have previously made applica-

tion for them, at the cost of making such copies. *Provided*, That the author of a paper shall in no case be deprived of his right to give copies of the same to any one he chooses, before it is read, or afterward; but if such paper is published unofficially prior to the meeting at which it is to be read, it shall be considered as withdrawn by the author, and shall not be presented for reading or discussion as a paper of the Society."

The President: You have heard the amendment as proposed, gentlemen. The Secretary has copies for distribution, and I presume that in so important a matter you will want to read them over carefully before passing the matter to vote. The subject is, however, before you for action in any form in which you choose to deal with it.

Mr. Bayles: As the amendment was seconded at the last meeting I presume it is debatable.

The President: It is open for debate.

Mr. Bayles: Rule XL, as it stands in the present code of rules and regulations, was adopted at my instance, as an amendment made by me at the meeting for organization. But it does not work satisfactorily; the difficulty being that authors of papers, while they may desire as great publicity as can be had by repeated publication, find it too difficult and laborious to make many copies of their manuscript, and it goes into the Secretary's hands as a single manuscript, and no time is afforded for copying. The amendment that is offered to that rule, which the Secretary is distributing, seems to me desirable from three points of view. First, from the standpoint of the author. The engineer who works up a subject and reads a paper on it, desires to give that information to the public as broadly as possible. He has no desire to have it locked up in any way, nor to limit its publication to any one journal, however favorable he may be to that particular journal. From the standpoint of the Secretary, it is eminently desirable, because there is almost always more than one application for a paper, and he cannot discriminate between reputable newspapers as to whom he should give it to. It makes a pressure on the Secretary which is embarrassing at all times. From the standpoint of the press, it is eminently desirable, because it obviates that scramble which is unfortunately unavoidable when three or four newspapers are after a manuscript, and whichever newspaper obtains it, does so generally through superior enterprise. The object of this amendment is simply to open the door wider and place all newspapers on an equal stand-

ing in the Society, and I conceive that the work of the Society will be greatly promoted thereby. On the present basis, if I obtain a paper, others equally entitled to the privilege are deprived of the chance to print it. I have no desire to deprive them of that privilege nor to be deprived of it. Consequently, I offer this amendment with the object of placing the press on an equal and fair footing in regard to the proceedings of the Society.

PROFESSOR SWEET: We have heard the side of the press in regard to this matter. I should take the other side of the question. One might have a choice as to which paper he preferred to have his article appear in, and this deprives him of that choice. One paper might be able to publish the entire proceedings of a meeting, and if it should do so, other papers, which might be better mediums for the publication, would not be likely to publish them afterwards. It seems to me I should prefer the rule as it now stands, unless it can be shown to work better for the members as well as for the press.

MR. C. J. H. WOODBURY: In looking over this amendment, two or three questions suggest themselves. By the amendments proposed to Article XI., the Secretary is required to furnish members with printed copies of papers to be read at a meeting. Most of the members of this Society are very busy men, and it would be among the improbabilities if not among the impossibilities of life, for them always to furnish copies of their manuscript, in time to have them printed and put in form. It seems to me that the carrying out of the amendment would hardly be practicable at the present time. Perhaps writers at times will choose to discriminate as to who shall have papers. I do not think we should adopt any amendment which would tie up our right of private judgment in such matters. What Mr. Bayles said is true, that it is of advantage to both the Society and its members individually, to have the proceedings of the Society widely reported by the papers; and I should certainly be in favor of a course that would secure the widest publicity, but I think, at the same time, that members should have some discrimination in the matter. I therefore move to amend the proposed rule by inserting the words "with the author's consent," before the sentence beginning, "copies of the drawings."

MR. BAYLES: I see no possible objection to that as an amendment. From my standpoint, I certainly do not want to deprive

an author of the right to keep a paper from me if he chooses. I accept Mr. Woodbury's amendment with pleasure.

The President: The amendment being accepted, the members will consider those words inserted in the copies they have in their hands.

Mr. C. T. Porter: If I understand the amendment correctly it applies only to the copies of drawings.

Mr. C. J. H. Woodbury: I meant to annex it to the preceding sentence, before the word "copies."

Mr. Bayles: There must be some such clause as that introduced, because authors sometimes read copyrighted papers, which they are not willing to have printed. I know of such a case in the Society now.

Mr. W. J. Root: It seems to me the amendment does not reach the object aimed at. It says the Secretary shall have printed copies of the papers, and shall give the same to representatives of such newspapers as shall desire them, etc. Should not the sentence which is proposed to be inserted precede that, saying that with the consent of the author copies may be distributed to members and representatives of the press?

The President: "With the author's consent" might be inserted before the word "shall," to make it precise, I suppose, as Mr. Woodbury desires. Then it would read:

"The Secretary shall have sole possession of papers between the time of their acceptance by the Council and their reading, together with the drawings illustrating the same; and at the time of said reading he shall have printed copies for distribution to members present, and, *with the author's consent*, shall give the same to representatives of such newspapers as desire them for unofficial publication, in whole or in part."

Mr. C. T. Porter: I move to substitute *may* for *shall*—"*may* with the author's consent," etc.

The President: Mr. Porter proposes that instead of *shall give* making it mandatory, we put *may give*. Is that accepted by Mr. Bayles?

Mr. Bayles: Yes, sir.

Mr. J. Bailey: I would like to ask what difference it makes whether the representatives of newspapers ask the Secretary to give them for publication the copy of a paper or not. I certainly should not ask the Secretary to do anything of the kind.

Mr. Bayles: I do not quite understand the point of the gentleman who just spoke.

Mr. J. Bailey: I will endeavor so make it as plain as possible. If a paper is printed and distributed here to-day to all the members present, what difference does it make to the author or to anybody else whether the representatives of newspapers present ask for a copy or not? It is published; it is open for every one, and can be published by anybody who chooses.

Mr. Partridge: I think there is a little more that can be said on this question. Perhaps some members misapprehend the object of societies in general, or of this Society in particular. It is in a very large degree to make themselves and their work known, and to get their ideas not only before their fellow-members, but before the world; and in that view of the case anything which adds to their reputation is of monetary value. I presume that is the foundation of this amendment. Certainly anything that restricts in any way the publicity of their productions is an injury.

Mr. Bayles: I wish to say that I cannot conceive of any inducement large enough to prevail on me to publish the entire proceedings of the American Society of Mechanical Engineers. A great deal of the matter furnished here is of a strictly technical interest. It is not in any sense popular. The most that I would care to do with such papers read here is to give a popular summary of them; not to print them in full. An arrangement could not be made with me by the Society to take these transactions as an entirety and publish them. The same privilege that is open to us in the matter is open to other journals. If they choose to be enterprising in the matter they can anticipate us, and they are welcome to, because I shall go on and publish what I publish just the same, without reference to whether it has been published by any one else or not.

A Visitor: Does the Society part with its control, sir, because the papers are printed? Are they printed for the newspapers or for members?

The President: As a matter of fact under the regulations, the procedure is simply this: An author prepares a paper and forwards it to the Secretary of the Society. That paper goes before the Council, or, if the Council has not a session between that time and the time of the meeting, it goes to the Publication Committee. The Publication Committee examine that paper and determine whether it is of a character to justify their allowing it to be put on the list for reading. It is then trans-

ferred back to the Secretary, who holds it as the custodian for the Society. Nobody has a right to look at that paper or to touch that paper, until it comes before the Society. The Secretary holds it, not in his own right at all, but in the right of the Society, and if the right is not conveyed by the Society to any individual or any body, the Secretary has no right to do anything with the paper, but to keep it until it is called for at the meeting of the Council or that of the Society. That is the strict rendering of the rule as I should interpret it. Reading the closing paragraph on the slip you have in your hand, nobody can fail to understand what the rule of the Society as it stands to-day means.

Mr. Bayles: The recognition of the rule as it stands is a good thing; but that is not in any respect in accordance with the practice of the Society; because a majority of the papers read at this meeting are read from newspaper proofs, showing that they are set up in advance and are awaiting publication. I have myself seen several copies of papers in proof slips from newspaper offices, and I should personally be happy to extend the same assistance to any member who would like to read his paper from the proof, as I have done. I should like to see the rule in accordance with the practice of the Institute of Mining Engineers and the Society of Civil Engineers, and of foreign societies, all of which encourage by every means in their power the broadest possible publicity for papers read at their meetings.

Mr. J. Bailey: I want to say a word in favor of monthly publications. There are papers published once a month, some of which will not come out for three or four weeks from the present time. I will mention one instance, the *Franklin Institute Journal*, which published a paper read by our President. I want to know if the *Franklin Institute Journal* would have taken that up if it had been published in a weekly paper.

Mr. Rae: There is one view of this amendment to which I think attention should be called. It might give rise to the idea that no papers, excepting such as were printed and distributed before the meeting, should be read to the Society. I do not suppose that is the intention, but at the same time the amendment might be so interpreted; and, inasmuch as many papers come in late, I would suggest that in the tenth line between the words *he shall* and *have, when possible* be interpolated. Then it would read: "He shall when possible."

Mr. Bayles: That amendment seems to me to be quite un-

necessary. He cannot print papers he has not got, and the practicability of his doing so depends on his having time to do it.

Mr. Rae: Still I think my suggestion holds good. The amendment might be so interpreted as to exclude all papers not printed.

Mr. L. B. Moore: It seems to me very clear, in view of the friendly relations which exist between the various newspapers which are interested in the affairs of the Society, that if Professor Thurston, for instance, or Mr. Porter, prepares a paper and should say to one, two, three, or half a dozen journals, who apply to him for the privilege of publishing that paper, that he would be very glad to have them do so, in the majority of cases those gentlemen could easily arrange to publish the paper on the same day or in the same week. I know of instances where papers have been held back for some six to eight weeks out of courtesy by newspapers which had them in hand, but did not publish them, because the author desired to have them appear in a monthly journal which could not publish them before that time. It seems to me that the rule as it stands can be made to work very harmoniously and very pleasantly by a very little co-operation.

Mr. Partridge: The Society has little or nothing to do with the newspaper side of the case. As the rule stands now, it is well enough for the newspapers, but it is not well enough for the Society; and even if one newspaper, at the present time, should publish the whole proceedings, for another newspaper to refrain from publishing that which would be of direct interest to its readers, would rather argue that the big newspaper lapped completely over the small newspaper's circulation. No newspaper man believes that, and that question has no weight. It has no weight in England where papers read at a meeting are perhaps all published in one issue of one journal, and then other papers publish that which is of interest to them, as suits their convenience. The newspaper side of this question has no business in this Society. The point from which the Society should view it is what is best for the members and best for the promotion of the interests of the Society and of the individuals composing it. If the rule enlarges the audience which the members have, it is good. If it does not, by all means alter it, so that the audience of the Society shall be the largest possible.

Mr. Wolff: It seems to me that the authors of papers which are considered worthy of presentation here should be fully com-

petent to decide as to their publication. I think that most of the members who contribute papers to this Society find very great difficulty in finishing their papers much before the meeting of the Society. I know, in my own experience, of papers that were finished just before the meeting of the Society, and which, under the amendment now proposed, could not have been read at the meeting at which they were read. Again, as to this question of multiplying publications, I suppose it is well known that such periodicals as *Van Nostrand's Magazine* and the *Journal of the Franklin Institute*, refuse even good papers, in case they have appeared in other journals. Now it may not be right for these monthly publications to pursue such a course; but, on the other hand, the author may have his preference for a smaller circulation in the monthly papers to a larger circulation in the weekly papers. He may desire to reach that class of readers to which the monthly papers go. The author of a paper should at least have the right to say in which publication his paper shall appear first, and then other publications can reproduce it if they choose. It seems to me that, officially, the newspapers have nothing to do with the papers at all, except when they have appeared in the *Transactions*. A paper, I think, belongs to the author and the Society jointly. The Society publishes it in the *Transactions*, and the author may publish it wherever he chooses, besides that.

The President: The amendment now stands with the word *may* substituted for the word *shall*, near the middle of the paragraph. The words, *with the author's consent*, are inserted in the same paragraph, in order to give an author control of his paper; and it has been proposed that the words *when possible* be inserted in the sentence which, as it now stands, makes the printing of a paper by the Secretary mandatory.

Mr. Bayles: I would rather not accept that amendment, because it is always possible to do just what the motion calls for. If the Secretary gives notice that he wants the papers a certain number of days before the meeting, he will get them. If this be made a rule there will be no more trouble in conforming to it than there will be in conforming to a rule about the hour of meeting, or any other rule.

Mr. Rae: I withdraw the amendment.

A vote was then taken, and the amendment of Mr. Bayles was rejected.

Mr. Bayles: I would like to give notice of that amendment

to the rules for consideration at the next annual meeting, believing that by that time the feeling will have considerably changed, and that it will then pass by a large majority.

Mr. Hewitt: The ballots are now ready. I would like to state, before announcing the result, that one of the ballots had a line drawn diagonally across the entire list of names, which we understood as a rejection of each one.

The result is:

Total ballots,	177
For President.	
Robert H. Thurston,	176
For Vice-President.	
E. D. Leavitt, Jr.,	174
William P. Trowbridge,	173
Charles E. Emery,	173
For Managers.	
Allan Stirling,	175
George H. Babcock,	175
S. W. Robinson,	174
James W. See,	1
For Treasurer.	
Lycurgus B. Moore.	

The President: The ticket is declared elected. We have before us ballots for new members. Ballots were sent out under the rules, and these are ready for counting. What will the gentlemen do with those ballots?

Mr. Weightman: Was not the rule changed so as to require the Council to count those ballots?

The President: No, the Council were permitted at one time to make a count, in order to complete the catalogue, which was about to be published. The ballots are properly counted by the Society, under such method as it may choose. Will you have the Secretary count them, or wili you appoint tellers?

Mr. Weightman: I move that the Secretary be requested to count the ballots.

Agreed to.

The President: We are ready now to proceed to the reading of papers; and Mr. Root, who is one of the number recom-

mended by the Council for election, and whose name will go out with the ballots about to be sent out, has prepared a paper, which is thought by some of the members to be very interesting, on a subject that is of considerable importance,—screw propulsion,—and embodying, as he considers, some important and novel facts. I have been asked to make this statement to the meeting, with the suggestion that his paper be put at the end of the list of papers to be read at this meeting, and that his paper be called for at the close, if we have time to listen to it. It is a matter that cannot come under any of our rules; but a precedent for the action we now propose to take occurred at the Hartford meeting, where a paper was read by a gentleman not yet elected a member.

Mr. Hutton: If it is necessary, Mr. President, I will make a motion to that effect that the gentleman be requested to read his paper after the other papers have been read.

Agreed to.

The Treasurer then read his report.

Treasurer's Report.

96 Fulton Street, New York, Nov. 3d, 1881.

To the Society: Since my report, presented at the Altoona meeting, I have received funds as follows:

Initiation fees,	$345 00
Annual dues,	191 00
Life membership,	150 00
October interest on bonds in Safe Deposit Vault,	20 00
Total,	$706 00

Since the same report I have expended, to pay bills audited by the Finance Committee, as follows:

Printing and stationery account,	$68 35
General expense account,	177 71
Postage account,	38 66
Salary account,	298 58
Travelling account,	3 85
Total expenditures,	$587 15

Being a net gain to the treasury of $118.85 since last report.

There is still due to the Society, from the membership, initiation fees and annual dues, amounting to $190.

By way of general summary embracing this, together with the three previous reports which I have had the honor to make to the Society, I will state that, during my term of office just closed, I have received, from all sources, cash as follows:

Initiation fees,	$3990 00
Annual dues,	2368 00
Life membership,	450 00
Sale of papers to members,	20 18
Profit on $600 U. S. Bonds sold to meet expenses,	17 25
Interest on same, three months,	6 00
Interest on $2000 U. S. Bonds in Safe Deposit Vaults, six months,	40 00
Making the total cash receipts,	$6891 43

During the same term of office I have expended, to pay bills approved by the Society and audited by the Finance Committee, funds distributed under the following heads:

Engraving account,	$26 95
Travelling account, Secretary's expenses,	18 93
General expense account, including the following prominent items, viz.: Rent of hall, $220; hotel headquarters, $30; stenographer's fees, three meetings, $253.40; carpenter's bill for boxes, etc., $40.11; blackboards, crayons, etc., $70.65; omnibus hire, $60; in all,	796 49
Salary account, the items being: Clerk hire in March, 1880, $13.75; one year's salary of Secretary, $1500; copying and draughtsman's pay in Secretary's office, $48.58; total,	1562 33
Printing and stationery account, including bill for printing papers read in November, 1880, amounting to $671.50; in all,	1166 34
Postage account,	178 81
Making total expenditure to date,	$3749 85
And leaving cash on hand,	3141 58
In bank,	864 08
Cost price of $2000 U. S. 4 per cent. Bonds, in Safe Deposit Vaults (now worth about $45 more than here valued),	2277 50
Total cash on hand,	$3141 58

All bills, duly audited by the Finance Committee and presented to me, have been promptly paid. As above shown, I have kept a liberal deposit lying idle in bank for some months, in readiness to pay bills for printing the transactions of the meet-

ings held at Hartford and Altoona six and three months ago respectively, but am as yet unadvised as to when funds will be called for for that purpose.

Respectfully submitted,

LYCURGUS B. MOORE,
Treasurer.

THE PRESIDENT: I would say, gentlemen, that this report comes from the Council. The report of the Treasurer is made to the Council, and the Council are under the rule directed to make a report to the Society at each annual meeting, and this report is a report of the Council, and is to be received as such.

On motion the report was accepted.

MR. HUTTON: I think that it is only just, in view of the fact that the services of our Treasurer are so laborious, and have been so successful, that the Society should return a vote of thanks to the Treasurer for the way in which the funds have been administered during the year. I would make the motion that such a vote of thanks be tendered by the Society.

Agreed to.

MR. L. B. MOORE: It is probably well known that during the past eighteen months I have devoted a very considerable portion of my time to the task of collecting, taking care of, and disbursing the funds belonging to this Society. Giving my services, wholly without salary or thought of reward, I have contrived to do the work without spending a penny of the Society's funds for clerk hire during my entire term of office. When it is considered that this task involves keeping a complete set of books containing over three hundred accounts, together with almost constant correspondence, it will be seen that it must necessarily trespass heavily upon time that should be devoted to my own very exacting business.

Having been designated by unanimous vote, in the first instance, and desiring only to do my part in the work of building the Society up from nothing to an established position among the great engineering societies of the world, beyond the power of the belittling influences that had heretofore nullified all previous attempts to found a national association of mechanical engineers, I have hitherto been content to carry the burden of labor and responsibility which your kind confidence has placed upon me. Closely occupied as I have been, therefore, I had

hardly given a thought to the personal aspect of my relations to the Society, as one of its officers, and in this manner I allowed my name to be placed upon the recent ballot list.

But since this ballot list went out among the membership, or rather within the past few days, intimations have come to me from personal friends that a few persons are disposed to apprehend that my official connection may, perhaps, enable the journal with which I am personally identified to exercise an undue influence in the affairs of the Society.

That these feelings are entertained by but few matters little. It is enough for me to know that they are entertained.

Had I been discharging the delicate duties that should fall upon the Secretary of such a body as this, I could more readily understand the grounds of such an apprehension. In the mere care of the Society's funds, and, for example, knowing nothing of the papers which are to be read at any approaching meeting, except as the Secretary, by impartial written communication, notifies all journals in any degree interested in promoting the Society's welfare, it has seemed to me that there should be no rational ground for objection in the mind of any one.

But while deeply and sincerely grateful to the Society for the compliment of re-election which has this day been paid me, I cannot longer allow myself to sacrifice my own private interests while standing in the way of the Society's widest usefulness. Recognizing that in the future it will probably be better for this Society and the newspaper press to be wholly independent of each other, for the best interests of both, and promising to lose no proper opportunity in future that I may have to benefit the Society, I desire respectfully to tender my resignation of the office to which you have this day re-elected me, and to ask that you will at once appoint some one to whom I may account for the trust placed in my charge.

Professor Trowbridge: I think that on account of the services Mr. Moore has rendered to the Society, and the great interest he has shown in it, we might ask him to withdraw his resignation for a while, and unless he has personal reasons for wishing to be relieved, I am sure the Society would prefer to continue his services.

Mr. Weightman: In order to put it before the body, I would make the motion that his resignation be *not* accepted.

Seconded by Professor Trowbridge.

Mr. L. B. Moore: I think that is hardly a fair position to place me in. I am quite sincere in this matter. I do not make resolutions of that kind hastily, and I propose to stick to this. The Society is firmly established, and I see no reason why, under the circumstances, some one occupying a different position would not answer better.

Mr. Weightman: My idea in making this motion, having been one of the tellers and having counted the vote, and found it to be, with one exception, a unanimous vote, was that we could hardly do any more than request the gentleman to at least withdraw his resignation. I would shape the motion in that way that the Treasurer elect be requested to withdraw his resignation.

Seconded.

Mr. Wolff: I would like to add that the vote which the Society has given to-day, as reported by the tellers, is the best proof of the confidence which the Society at lar7e places in the Treasurer. So that I think anything coming to his notice of the character he mentioned is certainly not the expression of the Society. Indeed, the expression of the Society, as shown by the votes cast, is entirely to the contrary, being a vote of confidence in Mr. Moore's ability to continue in the discharge of his duties as Treasurer, and I feel that we have a right, since that confidence has been shown, to call on Mr. Moore to retain his position.

A Visitor: Speaking for the press, sir, I do not think that any of the contemporaries of the journal Mr. Moore represents have any fear of any collusion on his part with the Society that would injure them in any way.

Professor Trowbridge: I can well understand the position of the Treasurer of an association of this sort. It involves a great deal of extra labor, time, and responsibility. Every one who has held such a position knows what a large degree of responsibility attaches to it in the mind of a sensitive person. Now I should propose that we apply a salary to the position, which would relieve him of some of the drudgery of the bookkeeping of his office. I think it would be better to do that.

Mr. L. B. Moore: My position in the matter has no reference to any wish on my part to have a salary or allowance for clerk hire, or anything of the kind. I prefer that I be taken at my word and allowed to retire from the governing body. I feel that I owe the Society an apology for allowing my name to go on the ballot list. If I had known five weeks ago what I know

now, I should not have allowed my name to go on the ballot list.

Mr. Weightman: I think, from what Mr. Moore says, this is merely an outside matter,—outside the Society,—and that the vote that is passed already is a sufficient answer to anything touching him personally. We all know that nowadays, in every daily newspaper, there are all sorts of innuendoes, inserted to-day, only to be forgotten to-morrow. I think we would better request the Treasurer to withdraw his resignation.

Mr. L. B. Moore: I would like to round this matter up by making one statement, and that is that my action in making this request has not been owing to anything that has appeared in any newspaper. What I have heard has come to me from private sources, and it made me feel that I should very much prefer to have some one else occupy the position of Treasurer; some one who would perhaps be less open to objection than myself.

The President: The matter lies, of course, between Mr. Moore and the Society. If Mr. Moore can retain the position and give the affairs of the Society the conscientious care I know he has given them, it remains simply for the Society to lay the request before him as it has already done. If he is unwilling to remain in the position, of course that is the end of the matter. I would like now officially to say, that in my personal relations with Mr. Moore, I never have found him lacking in any respect in the care of the interests of the Society, and I have never seen anything that could justify any aspersions upon the Treasurer from any direction. I never have known, in my official connection with this and other societies, a more conscientious, careful, scrupulous officer than our Treasurer has been. As presiding officer, I would say that it would be a great relief to other responsible officers if Mr. Moore could be induced to retain his position. Understanding, probably, better than most members do, how Mr. Moore feels, and what are the circumstances, possibly, that induce him to take the step, I can appreciate the situation better than others, and I should hesitate, and perhaps positively decline to take any action pro or con in the matter. It must be left entirely, in my judgment, to the Treasurer himself; but with the other members of the Society I hope sincerely that he will be induced to reconsider his proposed resignation. In the event of Mr. Moore's insisting on the acceptance of his resignation, we are called upon to elect a Treasurer; that will come

properly under the rules before the Council. The Council will be called on, after the annual meeting, to elect an officer to fill this vacancy. And it will be the duty of every member of the Society to look about for some one to take that place, if it becomes necessary to elect another Treasurer. I do not know of any one better qualified than Mr. Moore to fill that place. I hope, if Mr. Moore should insist on resigning, that the members will assist the Council by their advice.

Mr. F. B. Allen: I hope no action will be taken by which it would seem that we would accede to Mr. Moore's wishes regarding his resignation. While Mr. Moore would have vastly more time to himself if he were to resign the treasurership, the Society are largely the gainers by his holding that office, and cannot spare him; and I hope that, in consideration of the full vote of confidence that has been given, he will be persuaded to reconsider his determination and accept the office for the ensuing year.

The President: I presume Mr. Moore will take the matter into consideration, and inform us before the session has closed what is his determination. I believe there is nothing more to be done now, so we adjourn to meet at seven o'clock, if that is the pleasure of the Society.

Evening Session.

The meeting was called to order at 7.30 p.m.

The President: I wish to remind the members that the Secretary has a set of cards of identification, which are certificates that the holders are members of the Society. Those gentlemen who were not here early in the day will please call upon the Secretary and get those cards. You will often find them useful for introduction. They are used by almost all other societies. The Secretary also has members' badges for those who choose to take them.

The meeting then adjourned till the following day at 10 a.m.

SECOND DAY'S PROCEEDINGS.

The meeting was called to order at 10.45 a.m.

Mr. J. B. Root read a paper entitled "Screw Propulsion."

At the conclusion of the paper and discussion thereon the Society went into executive session.

THE PRESIDENT: It is in order for the Society to determine what it will do with this paper.

MR. E. D. LEAVITT, JR.: I move that it be laid on the table.

THE PRESIDENT: The proper motion would be, I presume, that it be printed or not printed. Permission has been given to read the paper, and the next thing to be done is to decide whether it shall be printed or not. If not printed, it simply lies upon the author's hands as before, as an unofficial paper. If printed, it goes into the *Transactions*.

MR. WOLFF: In case it is not printed, what disposition will be made of the discussion that has taken place?

THE PRESIDENT: It drops out as entirely unofficial.

PROFESSOR SWEET: In order to bring the matter before the Society, I move that the paper be printed.

MR. WOLFF: It does not appear to me right that even in case the paper were not printed that the discussion should drop out. That is a part of the regular proceedings.

THE PRESIDENT: The discussion is part of the paper.

MR. WOLFF: I think that under those conditions, if for no other reason, the paper ought to be printed in order to have the discussion; not that I mean to imply that it would not be a good thing to have the paper printed on its own account.

THE PRESIDENT: If the paper is not printed there will be no basis for the discussion.

MR. F. B. ALLEN: In seconding the motion for the printing of the paper, I do so with the object that the paper may receive full investigation.

The motion that the paper be printed was agreed to.

THE PRESIDENT: The paper will be printed with the discussion. I presume Mr. Root will furnish the facts asked for at the next meeting.

I would announce that all those who were balloted for are elected, and the notifications of their election have been prepared by the Secretary and are here. If any gentlemen on the ballot list are in the house, they can come forward and receive the official notification of their election.

The meeting then adjourned till 2.30 P.M.

Afternoon Session.

The meeting was called to order at 2.50 P.M.

The President: The Council has held a meeting in accordance with the Rules, and it was found necessary to provide for the election of a Treasurer and Secretary for the coming year. That requires for action a meeting of the majority of the members of the Council, including Vice-Presidents and managers, and a special meeting of the Council will be called for the consideration of the resignation of the Treasurer, which is necessarily accepted, as it is pronounced final by the Treasurer, and the election of his successor, and the election provided for in the Rules, of a Secretary.

The action of the Council includes a matter which requires to be brought to the attention of the meeting. It directs me to report to the Society a recommendation of the reconsideration of the motion made this morning that the paper presented by Mr. Root be printed with the discussion, and that instead of that paper being printed as proposed, that it should be referred back to its author with the request that he present with that paper such facts as may have been determined by his own experiments, at a later meeting of the Society. That is the recommendation of the Council. The gentlemen have heard it, and of course will act according to their own pleasure in the matter. It is in order to take action upon that at once.

Mr. Hutton: I would move a reconsideration of the action of the Society.

Agreed to.

The President: The suggestion of the Council is that the paper be referred back to the author with the request that he present, at a later meeting, the facts determined by his experiments; the paper being regarded as incomplete.

Mr. Hutton: I will make a motion to that effect.

Mr. J. B. Root: I was invited to read this paper before the Society and I have done so, and it has been criticised to some extent. Now I gave in that paper certain facts which I say have been demonstrated by experiment—two leading facts. One is that the velocity of the vessel exceeds very largely what the velocity of the screw and the pitch of the screw would account for. I give the reasoning upon the fact, and show why it is so. Another is that there is a certain amount of constant velocity of the water through the screw, which is constant from the first starting

of the screw, that diminishes the reaction as heretofore calculated upon the horizontal screw. From these two facts, which I consider to have been verified by experiment, I go on to show why such a result takes place. Now I am not aware that it is necessary for me to go into the minutiæ of the results that are produced by the engine, by the boiler, by the wind, by the tide, by the amount of resistance, by the size of the boat, and spend the whole of my time for the next year to come, to figure out all these minutiæ. The value of it, if it has any value, lies in substantiating the two leading facts that I have set forth in the paper, and those were the only ones that I intended to offer. I claim that those two certain results in economy have been verified. They are so large that there is no necessity for splitting hairs about them. If this Society does not wish to publish this paper I can publish it myself. As to the substantiation of those facts that I named, I consider that the paper is of such a character as to place the subject before the minds of engineers who are capable of appreciating it. That is all I wanted to do, in the hope that it would lead to a discussion upon the points in the paper that I considered of importance.

A Member: The Board of Managers act as censors on papers presented. Are not they properly qualified to judge? Is not that their duty?

The President: That is their duty in regard to papers presented by members of the Society. Mr. Root is not a member of the Society, and the action is taken by the Society and not by the Council. There is no rule that allows cognizance of any papers not presented by members. Mr. Root has been recommended by the Council for membership, and he undoubtedly will become a member before the next meeting. It was proposed by members and agreed by the Society, that Mr. Root be requested to read this paper now, instead of deferring it until he should become a member. So that the case as it stands is simply that a gentleman who is not a member of the Society, but who we hope will become one, has read the paper which he was invited to read. The Society has heard the paper, and voted that it be printed with the discussion. The Council have since recommended that the Society reconsider that action, and decline to print it in the *Transactions*. They have only a right to make such a recommendation, and their recommendation receives such weight as the Society chooses to give it. If the Society chooses to adhere to

the action taken already, it goes on the Society's files and becomes a portion of the Society's transactions. If they withdraw from that position, the paper has been read before us by a gentleman who has no connection with the Society, and like any other matter of interest to which we might give our time, it has simply performed its office of interesting, instructing, or entertaining the members. The Council recommend that the paper be referred back to its author with the request that he present with it facts determined by his experiments, as they regard the paper as incomplete. And I would say further, that in the course of discussion the impression among the members of the Council was simply that this paper was a presentation of the main facts determined by Mr. Root, with his own ideas, and with a certain amount of theoretical treatment which is of comparatively little value; while the power of the engine, the comparison of different screws, the comparison of power required under two sets of conditions, have not been presented, and therefore there was no basis on which to form a judgment of the real value of the work done by Mr. Root. It is requested that when he becomes a member of the Society he present a complete statement of the case, the Council regarding the statement already made as incomplete.

A MEMBER: Is there any way in which the paper can be printed by the Society, with the discussion thereon, without the Society assuming an official obligation, leaving it to be determined whether Mr. Root has made out his case or not?

THE PRESIDENT: All papers presented to the Society are presented with that understanding. The Society never indorses a paper presented by a member, nor does any society, unless by special action. But under the rules of this Society anything printed by the Society is printed as the private opinion, the private work of the author. The Society does not presume, in any case, to judge pro or con of the intrinsic merits of a paper except so far as to determine whether it may be read.

MR. WOLFF: Under the circumstances, after Mr. Root has been invited to read his paper, and it has been discussed here, it seems but just that the paper should be printed with the discussion, since the Society takes no responsibility. If any mistake was made the Society made it when they asked Mr. Root to read that paper, without first perusing it. But after the Society has invited Mr. Root to read the paper, and after he has read it, and it has been discussed, and the Society has decided to publish it, and since it

only remains the private view of Mr. Root, not indorsed by the Society, I cannot see that it is just to take any further action on it. Let it be published as the Society decided this morning.

Professor Sweet: As a consolation to Mr. Root I would like to bring up the historical fact that Henry Bessemer read a paper before the Royal Society describing his invention for making steel. The paper was ordered to be published; but before the publication he failed to confirm his statements by trial, and the paper was voted to be stricken from the rolls. Now, if that is to occur here, Mr. Root can console himself, certainly.

Mr. J. B. Root: I have confirmed all that I have claimed in that paper—except the degree of economy, which is based upon theory, in a measure,—by trial, by experiments that have cost me, to say the least, five thousand dollars, running through two or three years of time. I have developed valuable features, and I bring forward the indorsement of an engineer than whom, I think, none stands higher, Mr. Charles E. Emery. Now there comes up this opposition; why is it? Am I required to go to work and build a steamship to cross the Atlantic and make experiments with it to satisfy this Society? Is that what the Society wants?

Mr. Le Van: I shall venture to ask Mr. Root to retire. I am about to vote for the printing of the paper; that is, to let it stand just as it is now, but I have heard several members say that they would have voted against the printing of the paper only for Mr. Root's presence. I would, therefore, ask Mr. Root to retire while this question is decided.

Mr. C. T. Porter: I want to say that I am sure no member of this Society has any feeling in the matter, except one of the utmost friendliness and kindness toward Mr. Root, and the disposition that the matter as presented by him should have all justice given it. We treat it as we would treat anything else, without regard to any individual whomsoever. I am sure that nothing can be further from the fact than that any prejudice as to theories enters into the matter. I refrain from voting one way or the other in regard to publication on account of the part I took in the discussion. I think I should vote to adhere to the action already taken, however.

Mr. J. B. Root: If the ideas advanced in that paper make as great a commotion in the art of steam propulsion as they have in this Society, I think it will be a success.

Mr. Root then withdrew, and a vote being taken, the motion was declared lost.

The President: Then if no further action is proposed, the action of the Society as taken this morning stands. The paper goes upon the *Transactions*, and is printed with these discussions. That is understood.

Mr. Le Van: I propose at the next regular meeting to offer a resolution to change the by-laws. I only want to give notice of it now. In the meantime I will send in a written communication, stating what I propose to do. It is in regard to the time of the regular meeting. I want to change it from the first Thursday in November to some other date.

The President: I have to announce the decision of the Council calling the next meeting of the Society in Philadelphia. The next meeting of the Society will take place in Philadelphia, on the second Wednesday in April. A formal call will be issued by the President in the usual manner.

The Council will call a special meeting in three or four weeks to consider the resignation of the Treasurer, and to consider the election of a Secretary, and in the meantime the members of the Council desire that the members of the Society should present their opinions with the greatest freedom as to candidates to fill these offices. We want to get the right man as Treasurer, and we want to keep him.

Professor Trowbridge: I would offer the following resolution:

"*Resolved*, That the thanks of this Society are due to the following-named gentlemen for the courteous invitations extended by them to visit the works of engineering interest under their charge, viz., C. H. Delamater, Esq., Brevet Major-General John T. Newton, U. S. A., Isaac Newton, Esq., the Hudson River Tunnel Company, the New York & Brooklyn Bridge Company, the President of Columbia College, Commodore George H. Cooper, U. S. N., Thomas F. Rowland, Esq., Franklin K. Hain, Esq., the Cold Storage Warehousing Company, the Pennsylvania Railroad Company, The Department of Docks; and further, that the Secretary be directed to convey to them this expression of the Society's appreciation of the compliment."

Agreed to.

Mr. C. T. Porter: I beg to offer the following resolution:

"*Resolved*, That the Secretary be directed to convey to Mr. and Mrs. David Williams of New York the thanks of the Society for the very enjoyable entertainment given by them to its members on the occasion of the Annual Meeting of 1881."

Agreed to.

MR. RAE: I wish to offer the following resolution:

"*Resolved*, That the thanks of this organization are due to the officers and members of the American Society of Civil Engineers for the many courtesies extended during the past year, and especially to the Secretary thereof for advice, assistance, and encouragement so grateful and helpful to the inaugurators of a new enterprise. Further, that the Secretary be directed to communicate the sense of this resolution to all whom it may concern."

THE PRESIDENT: It is a duty to say, that from the beginning of our attempts at organization, the Secretary of the American Society of Civil Engineers, and the Council of that Society, and those members of it who have been consulted in the matter, have shown the greatest kindness towards us, and the greatest interest in our welfare, and whenever a Council meeting has been proposed to be held in the city of New York, the management of the American Society of Civil Engineers have tendered the use of their rooms to us, and have always given us the room of their Board of Directors for the purpose of such meeting; there have been many other ways in which they have assisted our officers in the performance of their duty. It is a matter which of course could not come to the Society except just in this way. They have been unable to tender the Society special courtesies, but they have done a great deal for the officers, and especially for the Council.

The resolution was agreed to.

MR. LE VAN: I would like to give notice, also, that at the next meeting I shall submit a resolution to the effect that the length of papers be limited. I think the time for reading papers should be defined, and also that every paper should be printed in advance, so that members can examine the subject thoroughly, and be prepared to discuss it in an intelligent manner.

The resolution of Mr. Le Van is as follows:

"*Resolved*, That each paper intended for presentation and discussion at any meeting of this Society shall, if it pass the Board of Councils, be printed, with suitable illustrations, 'subject to revision,' and mailed to each member at least six weeks prior to the meeting for which it is intended; and shall also be distributed at said meeting, at which it shall be read by title *only*. Discussions thereon shall then be in order; discussions to be stenographically reported and appended to the revised copy of the paper, which revised copy, with the report of the discussion, shall constitute the authorized edition of the paper, and shall be part of the published *Transactions* of the Society."

The Society adjourned to meet in Philadelphia on the second Wednesday in April.

PAPERS

OF THE

NEW YORK MEETING, 1881.

L.

OUR PROGRESS IN MECHANICAL ENGINEERING.

THE PRESIDENT'S ANNUAL ADDRESS.

BY ROBERT H. THURSTON, PROF. MECH. ENG., STEVENS INST. TECH., HOBOKEN, N. J.

GENTLEMEN OF THE SOCIETY:

It is impossible, in the limited time that must be allotted to the President's Annual Address, to do more than glance rapidly over the broad field of mechanical engineering, selecting for study the more prominent and more important departments, and very briefly noting what is their present state, and how far improvement has progressed during late years. The direction of movement to-day becoming known, and the character of the difficulties presenting themselves being ascertained, the way in which accelerated progress may be rendered possible, becomes more easy of detection. In many cases we shall find ourselves able to decide precisely where to look for such progress, and, in all directions, we shall find our exploration interesting, gratifying, and profitable. We will first examine those departments which supply us with our materials.

In that field to which we are apt to give too little consideration, notwithstanding the fact that it lies at the base of all our work, a field which—formerly cultivated by many of the greatest men that our profession has known—is now too generally neglected, while more seductive but less fruitful, and, on the whole, less immediately important departments are overcrowded with able workers, in that of the materials of construction, we are making steady progress on every side.

We are everywhere giving up the use of that expensive and perishable material, wood, and the weak and brittle minerals, and are substituting for them iron and steel.

Iron is slowly but steadily and inevitably being displaced by steel. Cast-iron in small parts is less and less used as steel castings become more and more reliable, and especially as the art of making drop-forgings of larger size and in more intricate forms

is perfected. Sheet steel, very low in carbon and other hardening elements, is becoming, year by year, more generally adopted in boiler-making; not because of its greater strength, for the stronger grades are always rejected by the experienced boiler-maker, but because of the greater uniformity, ease of working, freedom from cinder, and the durability of those grades which are well suited to such use.

A tenacity of *less* than 65,000 pounds per square inch (4550 kilos. on the square centimeter) and great ductility are demanded for this work.

In rods and bars, and for sheets to be used where mechanical forces only are present, we are getting steel which, with a tenacity of 80,000 pounds per square inch (5624 kilos. per square inch), stretches 25 per cent. before breaking, and we are sometimes given a grade very low in carbon, but high in manganese, which has 10 per cent. higher tenacity and equal ductility. In fact, we are apparently coming to a *manganese steel* as the metal for use in general construction.

In making alloys I have been able to show the existence of an alloy of copper, zinc, and tin of maximum possible strength and to point out approximately its composition, and my discovery has been confirmed by other investigators, who have independently hit upon alloys closely related to this "maximum metal," and possessing properties of hardly less value. We now know that by carefully proportioning the constituents, by properly fluxing the alloy, and by special mechanical treatment, we may obtain brasses and bronzes having strengths undreamed of by earlier engineers. Tenacities of from 75,000 to over 100,000 pounds per square inch (5273 to 7030 kilos. per square centimeter) having already been attained.

The introduction of special alloys having extraordinary strength and uniformity of composition, as the phosphor-bronzes, manganese bronzes, and sterro-metal, indicates that workers in metal are beginning to enter upon the path long since opened to them by scientific research.

Dr. Fleitman's discovery of a method of making nickel malleable and capable of welding, and his similar improvement of commercial cobalt by the use of magnesium, is in itself important, and promises to lead the way to further progress.

In the application of the materials of construction we are learning some exceedingly important facts, the most valuable of

which relate to those most "precious" of the metals, iron and steel.

The effect of variation of temperature in the annealing of these metals, and in the hardening and tempering of steel has long been known. That annealed and unannealed wire differ widely in tenacity and in ductility, that very "mild" steel and good iron are softened by the very process which gives hardness to steel, are long-familiar facts, and it probably has been long known to many engineers that there exists a critical temperature, probably definite and fixed for each grade, at which the hardening of steel occurs. Passing this point in cooling the metal takes on its temper, but variations of temperature on either side that point produce no observable effect on its condition, however rapidly they may take place. This critical temperature has now been identified in certain cases, and may prove to be nearly the same for all steels.

The process of "cold rolling" has long been known to engineers as an exceedingly valuable method of enormously increasing the strength and elasticity of iron. We have now learned that it is applicable to the soft steels, and I think it will become certain ere long that its full effects may be obtained at any temperature below that critical point which defines the limit of molecular stability, as I have just stated it, in steel.

Lauth's process has been applied with equal success to certain alloys of copper and tin, by Sears, in the United States, and later by Rosetti, in Italy, and very extensively and successfully by Uchatius, in Austria. Tobin has cold-rolled bronzes, approaching the "maximum" alloy in composition, and has attained tenacities exceeding 100,000 pounds per inch (7030 kgs. per square centimeter).

The radical distinction which is observable in the behavior of metals under stress, and which leads to their division into what I have proposed to call the iron class and the tin class—in the variation of the normal line of elastic limits by intermitted stress—is becoming generally known. Engineers are beginning to perceive that that exaltation of the normal elastic limit, which is observable in the former class, is probably a valuable quality, one the existence of which may prove to justify the use of smaller factors of safety than have hitherto been thought allowable; and this leads to less expense in stationary structures, and to the elimination, to some extent, of stresses due to the inertia of moving

parts of machinery. The conclusion reached by many engineers, that moderate static loads may be sustained indefinitely by iron and steel are also to this extent sustained.

On the other hand we are led to the observance of more than usual caution in the use of metals of the tin class, including most of the brasses and bronzes, and to the use, in such cases, of higher factors of safety than are demanded in constructions of iron and steel.

Preliminary straining to secure an elevated initial elastic limit with relief of internal stress is likely to be of service in the applications of iron and steel, as *e. g.* by cold-rolling, by "frigo-tension" and "thermo-tension" and by wire-drawing, while it proves to be probably less effective with other metals.

The experiments made for the Prussian government by Wohler and Spangenberg during a period of fifteen years, and which have now been concluded eight years, are just becoming known to practising engineers, and Wohler's law, Launhardt's and Weyrauch's analyses of results are found valuable checks upon usual methods of proportioning iron parts of structures. It is becoming known that not simply the load to be applied, but the frequency and the method of its application, and the condition of the structure as determined by earlier strains, must be considered in settling upon its dimensions, and upon the magnitude of the factor of safety.

Nevertheless, in ordinary work we find that, as experience has taught us, these qualities are well covered by the factors of safety that have become generally accepted. The great value of these researches, and of the many others of the same kind which have been made in every part of the civilized world, is found to come of the ability now conferred upon the engineer to proportion with confidence parts exposed to exceptionally great and unusually variable stresses.

Perhaps the most important advance made in the use of materials in engineering has been the general introduction of systematic inspection, and of careful test of all materials used. Such inspection and test are now demanded by every well-drawn specification, and are carried out usually by trained and skilful inspectors.

The Pennsylvania Railroad Company, the Bethlehem Iron Company, and other well-managed establishments have even organized complete departments devoted to the examination and test

of all materials offered them, and often find that a single investigation repays the whole cost of the department for a period of years. So essential is that system found to be that I am frequently called upon to advise in regard to new "laboratories" in all parts of the country in which iron, steel, lubricating materials, and other supplies of value are to be systematically examined before purchase. This is not a mere matter of dollars and cents, however. Every engineer who has experienced the anxiety which comes of uncertainty in regard to the character of the material of a structure, in which a single defective piece may cause the destruction of the whole with enormous loss of time, money, and probably of life, will understand what good comes of a system of inspection and test that entirely relieves both conscience and pocket of responsibility and risk.

A method of inspection which, as I showed ten years ago, will safely determine the value of each piece, subsequently to be actually put into the structure or machine, is now slowly becoming adopted, and we may hope that soon we may confidently assert of each bridge over which we ride, of each machine upon the strength of which depends safety of life and property, that its every part has been proven, by actual test before use, to be perfectly safe. Now that the great testing machine at Watertown Arsenal, set up by the unfortunately defunct Board appointed in 1875 to test iron, steel and other metals, is at the service of the public, we may hope that such methods of test may hereafter become common, and that tests of full-sized parts of bridges and machines, made at private cost, may, to a limited extent at least, yield the knowledge that that Board would have more systematically and at less expense have made familiar to engineers, had its life not been terminated at the very beginning of its labors.

I have to thank General Walker, the Superintendent of the Census, for many valuable data, and not the least interesting of all is the report of Mr. Swank, on the iron trades, of which I am supplied with an advance copy. I know of no better gauge of the extent and importance of the work of the mechanical engineer than the production of iron and steel. These metals are worked up by the mechanical engineer and the trades associated with the profession, and the consumption of the raw material is the truest measure of the magnitude and value of our work.

The growth of the iron manufactures of the United States has all occurred since A. D. 1700, when there was not a blast-furnace

in this country, and principally since the year 1794, when the first steam-engine was erected in America, eighteen years after James Watt made the improvements that have given him fame, and that have given the world more wealth and comfort than had been accumulated during the many centuries of civilization historically known to us as preceding his time.

To-day, we have over 1000 iron and steel works in the United States, employing \$230,000,000 in capital, as against \$122,000,000 in 1870-1871, producing 7¼ millions of tons of iron and steel, just double the production of 1870, and employing nearly 150,000 men. The value of all products is not far from \$300,000,000, and wages amount annually to about \$55,000,000. In ten years Massachusetts has increased her product 65 per cent., West Virginia, 104 per cent., Alabama, 800 per cent., nearly, Georgia, 125, and Tennessee 125 per cent. Pennsylvania holds her place at the head of the list, producing 3½ millions of tons per annum; Ohio makes 1 million, New York, 600,000, Illinois, 400,000, New Jersey, a quarter of a million, and other States smaller amounts.

Since the year 1870, we have increased the weight of pig metal from 2 to 3¾ millions of tons per annum, or 84 per cent.; rolling-mills make 2½ millions of tons of rolled iron, an increase of two-thirds; the Bessemer steel manufacture has grown from less than 20,000 tons in 1870 to 900,000 tons in 1881; "open hearth steel" is now reported at about 95,000 tons, and this is an industry which was unknown in this country in 1871. Of crucible steel, we make 70,000 tons—a gain of 150 per cent. in the decade—and its applications are extending wonderfully, day by day.

But Great Britain still remains at the head of iron-making countries, turning out 8 millions of tons of pig iron during the year, an increase of one-third since 1870, and the increase still continues. The weight of Bessemer rails made has reached above 700,000 tons, and of Siemens Martin steel a quarter of a million tons per annum. Germany and France exhibit similar gains in amount of iron and steel made, and other countries of the world follow after.

Even Italy, famous as the home of the fine arts, yet disgraced by her neglect of the useful arts—the country of beautiful art galleries and of uncomfortable homes—has produced about 300,000 tons of iron ore, of which a small amount is there worked into finished iron. The artistic sense of her people is even here seen,

and her blacksmiths make architectural work in hammered iron which would have satisfied even Michael Angelo.

The introduction into open hearth steel-making of the Pernot furnace with its revolving saucer-shaped hearth, and of the Ponsard furnace with its modernization of the ancient process, are the latest steps in the improvement of steel-making apparatus; and the dephosphorizing process of Thomas and Gilchrist, by permitting the use of hitherto condemned ores, will prove a grand step in the reduction of cost of Bessemer steel, which must hasten greatly that inevitable change which will, ere long, replace malleable iron by steel in all of its myriad uses. Good mild steel can at last be made cheaper than good iron, and we are now entering fairly into the "steel age."

This is the grandest of all the industrial revolutions that have affected the iron trades; although its influence upon the prosperity of the nation and of all civilized countries cannot be yet estimated, we may be sure that it will be hardly less observable, and that it will be of hardly inferior importance to the world, than was the introduction of puddled iron a century ago.

We are using steel in every department of our work, and that most remarkable of all its many grades, Whitworth's compressed metal, is now at last coming into commercial importance; and of it are to-day made shafts for the largest steamships and ordnance that has no equal in strength and endurance. Any size desired can now be made in cast steel, and 100-ton ingots, shaped under 80-ton hammers, are the *pièces de resistance* of at least one European establishment.

The progress of art, directed by brain and sustained by energy, skill, and enterprise, is well illustrated by the changes which have taken place in our textile manufactures. According to Atkinson, a century ago one person in each family was compelled to work, day in and day out, nearly the whole year, to furnish homespun and dress-goods for the rest; to-day, such has been the progress in the introduction of mechanism and automata, that one day's work in the year will, on the average, be sufficient to enable each worker to supply himself with all needed cotton and woollen fabrics.

Speeds of cotton spindles have risen, during the two decades that my memory can follow the change, from 5000 to 7500 revolutions per minute. Looms then making 120 picks per minute make now, Mr. Webber tells me, as high as 160, and one hand

takes charge of from 25 to 50 per cent. more work. The "Slasher" dresser does *ten times* the work of the old machine, supplying 400 looms in place of 40, and demanding the attendance of only one man and a boy, instead of two men and ten girls. Pickers handle a ton of cotton per day in place of a half or five-eighths ton. The cheaply made turbine driving these mills has completely displaced the old costly vertical wheel, doing the work with less water and greater steadiness. Its efficiency has risen from 70 or 75 to 80 and 85, and sometimes to 90 per cent.

When the last generation was in its prime our factories were in operation twelve or thirteen hours; "Man's work was from sun to sun, and woman's work was never done." To-day man works ten hours, and woman is coming to a stage in which she will work where, when, and how she pleases. Then three yards an hour was the product for a single operative; to-day ten yards per worker are produced. In twenty years the annual product in cotton mills has risen from 2½ tons to 3⅝ tons per annum per mill-hand; wages have increased 20 per cent., and the buying power of the dollar has risen in much more than equal proportion, thus adding 50 per cent. to the comforts and luxuries of working people, permitting an increased number of happy marriages and comfortable homes, setting free the child-slaves of the mills, and turning them into the schools.

Where one hand then drove forty spindles, he now manages sixty; and every seven of the more than ten millions of spindles in operation works up a bale of cotton each year, and turns out a hundred dollars' worth of product. This product is supplied to the most indigent of our poor at a small advance on the one and a half cents for labor and equal sum for raw cotton, which are expended in the manufacture of the cheapest grades. A still more striking fact is the distribution of our cotton goods to distant countries. A single mill operative at Fall River, Lowell, or Providence makes each year cotton cloth enough to supply 1500 of the people who pay her wages by sending her tea.

In regard to woollen manufactures we have the same story to tell. All machinery has been speeded up, product increased, labor diminished, cost lessened, and machinery given greater automatism and higher efficiency both in making ordinary goods and in adaptation to finer grades. The manufacture has had a healthy growth, and the product is daily competing more successfully with the best of imported goods.

It is, I find, difficult, perhaps impossible, to make an exact statement of the extent of recent progress in the silk manufacture in this country. Power looms and automatic machinery have been introduced more slowly in this trade than in others. Yet progress has been made. New and improved apparatus is steadily displacing older forms; power machinery is taking the place of hand-worked machines—with some rapidity in mills working the coarser grades, and more slowly where the finest goods are produced.

The strength, durability, and finish of all kinds of silks are constantly becoming more and more nearly equal to the best imported. Indeed, the ladies assure me that some makes of American silk wear much better than any of foreign make yet seen in our market, and that several grades have a finish which compares favorably with the very best of European silks. In variety and in quantity of goods produced a steady gain is to be noted. The ingenuity of the American workman, aided by talent and experience, coming from the older silk-making provinces of Europe, seem likely to give to this manufacture a position of which its promoters may well be proud. Mr. Wyckoff, Secretary of the Association, reports June 30th of the current year, a production in the United States of nearly $35,000,000 in finished goods, by about 400 factories, employing a capital of $19,000,000 and over 30,000 operatives, whose wages amount to about $9,000,000 per annum. Good progress this for an industry often condemned as exotic, and one which has only become established within the remembrance of the majority of our members. A half million of spindles, running often 10,000 revolutions per minute instead of 5000 as a few years ago, and over 5000 power and 4000 hand looms make a large showing. Spinning frames occupy $\frac{1}{400}$th the space, and cost $\frac{1}{20}$th as much per spindle as in the earlier days of the trade, and the cost of the work has now become so small that $3.00 per pound spent in wages make silk costing $5.00 per pound into finished goods averaging $11.50.

In machine work generally the distinctively American idea of manufacturing as opposed to the old methods of making parts or mechanism in large numbers is steadily progressing, thanks to the ingenuity of mechanics like our colleagues, Pratt and Whitney and others, in devising tools especially designed for the production of definitely limited kinds of work. The same wonderful genius of invention which produced the Whitney cotton gin, the

Blanchard lathe, our screw machinery, and the more wonderful card-setting machine, has lately given us Sellers's automatic gear-cutter, the automatic turret-lathe, and a thousand and one machine tools hardly less remarkable in construction and efficiency.

Turning to the examination of the present condition of the railroad system of our country—that system which, binding State to State with lines of steel, is our strongest safeguard against political dissension and disunion—we find that changes are everywhere in progress under the direction of some of the ablest members of our profession.

It is now seventy years since Colonel John Stevens, in his memorable correspondence with De Witt Clinton, urged the adoption of a complete system of steam transportation on railways, and asserted that the time would come when "suits of carriages," as he said, would make their journeys, impelled by steam, with as much celerity in the darkest night as in the light of day, and stated that he "could see nothing to hinder a steam carriage moving on its ways with a velocity of 100 miles an hour," and that he "should not be surprised" at seeing them propelled 40 or 50 miles an hour. His contemporary, Oliver Evans, wrote: "A carriage will start from Washington in the morning, the passengers will breakfast at Baltimore, dine in Philadelphia, and sleep in New York the same day." But it was a generation later before these prophecies were credited; it was only when, fifty years ago, the introduction of railroads had an actual beginning.

To-day we have a hundred thousand miles of track laid down in the United States,—we have about one-half of the constructed railroads of the world. Trains here and in Great Britain make 50 miles an hour on schedule time, taking water from the track, and receiving and delivering mails without stop. A speed of 100 miles—Stevens's maximum figure—has been sometimes attained. Locomotives are frequently built weighing 50 tons; 70 tons has been reached, and every builder of engines is ready to guarantee the performance of an engine to draw 2000 tons 20 miles an hour on a level track. In coal consumption we have made some saving of late years. Three pounds of coal per hour and per horse-power is a usual amount, and a consumption of 2.6 pounds (1.2 kgs.) of coal, and of $22\frac{1}{2}$ pounds (10 kgs.) steam has been reported from recent locomotive tests.

The trapping of cinder and the reduction of intensity of com-

bustion by extending grate area are late improvements. The time will come, and it should have come already, when the nuisance of flying dust and cinder will be unknown. Comparative comfort has at last come to the weary traveller in our parlor and sleeping cars, and the greatest of all modern inventions in this department, the Westinghouse continuous brake and the Miller platform and coupler, have decreased the risk of journeying by rail to a merely infinitesimal quantity. A train which, when at full speed, can be stopped within its own length, is comparatively safe against the most serious of usual contingencies. Steel rails have driven out iron, and this superior metal is slowly and surely taking the place of its defective rival in boiler and running parts. It is an interesting fact that, while Bessemer steel is used for rails, open-hearth steel is coming to be as exclusively used for all parts of the locomotive.

The efficiency of the late styles of stationary engines is illustrated by figures like these: Corliss obtains a duty, as reckoned from figures recorded by my assistant at a recent 12-hour trial of his last Providence pumping engine, of 113,878,580, without reduction or allowances, and the average of several days' trial is 112 millions. Leavitt gives me data showing a duty for months together of about 105 millions, and obtains a horse-power with an expenditure of $16\frac{1}{2}$ pounds of feed-water per hour at Lynn and 16.23 at Lawrence. His Calumet engine with wet steam and but 200 feet piston speed, demands but 18 pounds (8.2 kilos.), and the Hecla hoisting engine is credited with the wonderfully low figure—16 pounds (7.3 kilos.). This, by the way, is the more remarkable from the fact that the jackets were disconnected. We thus sometimes meet with hints, apparently that we may do better work with an underheated than with an overheated cylinder jacket. The performance of the West-side pumping engines at Chicago, giving a duty of nearly 100 millions with lower heads only jacketed, is similarly significant.

This figure—16 pounds of steam per hour and per horse-power—may be put on record as the very best economy attained by our best engineers at the end of the decade 1870–1880. It is just double the weight which would be required in a perfect engine working steam of the same pressure at maximum efficiency. This leaves us still a fair margin for further advance in the construction of the engine. The steam-boiler is at a standstill; there is but little margin for gain in economy, but a large gain in weight

of steam supplied per pound of boiler may be expected when the tardily recognized advantage of forced circulation is secured.

Air and gas engines are here competing with stationary steam-engines, and, so far as I can see, in no other field. The compressed-air-engine, the petroleum-engine, and the gas-engine are all just now coming forward. I have no figures that I can rely upon except for the gas-engine, which sometimes consumes as little as 28 cubic feet of gas (1 cubic meter nearly), per hour per horse-power.

The solar motor proposed by Ericsson, the inevitably coming motor of some far-distant epoch, has, as yet, made no progress beyond the plans and experiments of the inventor.

I have nothing to report relative to either the development or the application of the theory of heat engines. The splendid labors of Rankine and the work of that most logical and classical, if less practical writer, Clausius, have so cleared the field that later investigators are driven into the exploration of minor departments of thermodynamics. The engineer is to-day seeking, with the aid of the physicist, to determine the facts and the laws governing the exchange of heat between the working fluid and its inclosing walls. This is for him to-day the greatest of the problems presented in this department.

The purely commercial aspects of steam-engine economy, familiar as they have long been to builders of expensive engines and to the more intelligent buyers, have barely attracted the attention of engineers generally, and have, as yet, apparently been entirely overlooked by all having a scientific standing with, I think, the solitary exception of that greatest of modern scientific engineers, Rankine. A year ago, in debate, I called attention to the fact that economy in fuel was but one among the many items of expense incurred in the operation of steam machinery, and that it formed by no means the greatest part of such expense in certain cases. The inference at once follows that commercial economy, affected as it is by all these items, must be studied with reference, not to cost of fuel simply, but with a view to making total expense a minimum. Rankine called attention to this obvious conclusion many years ago, and a paper presented by two of our colleagues at the May meeting in Hartford, extending Rankine's work, and applying his approximately exact method to modern engines, showed that commercial efficiency is often made a maximum with very much smaller engines, and lower rates of expan-

sion, than are found to give maximum economy of fuel. Such methods of determining size of engine will probably be generally adopted by engineers seeking the best interests of their clients. We are not, it is evident, to conclude from the results of the application of the Rankine method of determining size of engine and maximum commercial efficiency, that we are always to lose so large a proportion of the gain obtainable by further expansion of steam. We conclude, rather, that the engineer must direct his attention to improvements designed to reduce these counteracting wastes. He must find methods of rendering the machine, including boiler, automatic, and thus of reducing cost of attendance; he must find ways of reducing first cost, as by increasing speed and making smaller engines do the work, as by finding ways of building cheaply, yet doing good work, and of making lubrication less costly, or of doing away with it altogether. Automatic firing, or "stoking," automatic feeds, and automatic cleaning apparatus are already in use, as well as automatic regulation of the engine, of steam pressure, of point of cut off, and of chimney draft. All these improvements, when once made successful and thoroughly reliable, will come in effectively to aid the engineer in this direction, as well as the more direct advances in progress in the direction of reducing back pressure and of checking cylinder condensation, of increasing steam pressure, superheating, and obtaining by the use of all known methods of high ratios of expansion at maximum efficiency. The engineer and the physicist working hand in hand in the future as they have in the past—or perhaps the engineer-physicist—will sooner or later, following the paths pointed out by Smeaton and Perkins, and in our time by Corliss, Porter, and Leavitt, greatly reduce the now often broad margin between theoretical efficiency and commercial economy. When the engineer has once acquired the habit of gauging the value of an engine by the magnitude of its ratio of expansion at maximum efficiency, all this latter class of improvements will advance with increased rapidity, and when he sees that the magnitude of the ratio of expansion at maximum commercial economy is a gauge of his success in making steam-power useful, the first class of improvements and of inventions will similarly advance, while we shall gladly approximate to mechanical perfection, and this progress will occur at a rate which will be measured by the approach of the two ratios of expansion to the same maximum, finally both becoming nearly

coincident with the ratio of maximum efficiency of fluid for each given case.

The "compound" engine has become the standard type of steam-engine in use on shipboard as well as for stationary pumping engines. We still hear occasionally intimations that a counter-revolution and return to the single cylinder type of engine may be expected, but that change is not observable. The direction and extent of recent advances in marine architecture are readily noted. The proportions of length of ship to breadth remain, as during several years past, about 10 to 1 or 11 to 1, about 50 per cent. greater than has been considered by some of the best engineers as that giving highest efficiency. The Great Eastern, 680 feet long, of 83 feet beam, and measuring 25,000 tons displacement, still remains the largest ship yet built; but steamers are under construction for transatlantic lines 600 feet long, of over 50 feet beam, and fitted with engines of 10,000 indicated horsepower. A speed of twenty miles an hour in good weather throughout the voyage, making the distance from land to land in less than a week, may be expected soon to become usual. Double hulls and transverse bulkheads will make these great vessels safe even against the shock of collision with an iceberg.

Steam pressure has gradually and steadily risen since the time of Watt, when 7 pounds—½ atmosphere—was usual. To-day 6 atmospheres (75 pounds per square inch) is usual, and 7 atmospheres (90 pounds) is often adopted. Such pressures have compelled the general introduction of the simplest form of steam boiler; the cylindrical tubular boiler with large flues beneath the tubes, in which the furnaces are formed. Strength of flues is obtained by the use of heavy plates, sometimes flanged at the girth seams. "Mild" steel is here slowly displacing iron.

I have had occasion to remark, that in ordinary practice increase of steam pressure with correspondingly increased expansion gives, roughly stated, a decreased steam consumption, about in the ratio of the square root of the pressure; this seems true in recent marine engineering. During the past ten years steam pressure has risen from 4½ to 6 atmospheres—50 to 75 pounds by gauge—and the consumption of fuel per hour and per horsepower has decreased from 2 to 1.8 pounds (0.9 to 0.8 kilograms). Incidentally the area of heating surface has decreased from 4½ to 4 square feet (0.4 to 0.37 square meter) per indicated horsepower, that is to say remaining, as formerly, nearly 2 square feet

per pound of coal burned per horse-power per hour (0.4 square inches per kilog.); where, as in some cases, pressures of 100 and 125 pounds are adopted (7 to 10 atmospheres, nearly), somewhat further gain may be expected.

Increased pressure has been accompanied by increased speed of piston—from 300 to 500 feet per minute (100 to 150 meters, nearly)—and both causes have combined to reduce greatly the size and weight of engines. Formerly 500 pounds (220 kilogs. nearly) per indicated horse-power was a common figure; to-day one-half that weight is often noted, and in special cases, in which, as in torpedo boats, economy is not important, one-fifth, and even one-eighth those weights are said to have been reached.

Surface condensation is almost exclusively adopted, but the area of cooling surface is becoming less and less, and at the pressure soon likely to become general, the production of a vacuum may possibly cease to be desirable, as it is already known to be with unjacketed cylinders; and the non-condensing engine may yet displace the condensing engine at sea as it has on land, and on our Western rivers, where this comparison was earlier made and where the evil effects of cylinder condensation were earlier perceived. A still for converting exhaust and waste steam into feed water has already been used, and it must remain in use in all salt-water navigation.

Among the most interesting events of the years 1880–1881 have been the trials of the steam yachts "Anthracite" and "Leila." The first is a small vessel, 86 feet long, 16 feet beam, and 9 feet draught ($27 \times 5 \times 2\frac{3}{4}$ meter, nearly), fitted with a three-cylinder compound engine, and carrying 300 pounds steam (20 atmospheres, nearly) and upward.

Trials in London show these engines to have required but 1.7 pounds of coal (0.8 kilog.) and 17.8 pounds (8 kilogs.) of steam per hour and per horse-power. Cylinder condensation amounted to 30 per cent. in the first cylinder, and of this nearly three-fourths was re-evaporated before discharge from the third cylinder.

The same engines tested in this country require 21.6 pounds (10 kilogs. nearly) of steam per hour and per horse-power, the cylinder condensation becoming over 50 per cent., of which four-fifths was re-evaporated before reaching the condenser, the difference being probably due to a variation in the efficiency of the steam jackets and in speed of engines. This little yacht—the

smallest that ever crossed the Atlantic—should be remembered in history, quite as much on account of the lessons in engineering learned on board the little craft as on account of her far famous voyage.

The trial of the "Leila," under the orders of the United States Navy Department, was even more instructive than that of the "Anthracite." The "Leila" is a Herreshoff yacht, 100 feet long, 12 feet beam ($30 \times 3\frac{1}{2}$ inches, nearly), and measuring 37 tons. With a "coil" boiler, steam at 120 pounds at the steam chest (9 atmospheres), and driving the boat 15 knots an hour (17 miles), the engines developed 150 horse-power, using but 16.4 pounds of steam (7.5 kilogs.) per hour per horse-power. The cylinder condensation amounted to but 10 per cent.

An important deduction from the results of the trial of the "Anthracite" and the "Leila" is, that efficiency has little relation to size of engine when protection against cylinder condensation is secured, and this conclusion is further justified by the fact that some of the very best work has been done, where non-condensing engines have been compared, by small portable engines. Steam-engines of five thousand horse-power are equalled in economy by engines of one-fiftieth that power. A large difference in magnitude seems more than compensated by a moderate difference in steam pressure. We may conclude that high steam pressure cannot be expected to give great economy unless employed intelligently. The highest pressure may prove least economical when the engineer neglects to provide against loss by cylinder condensation. In the cases of the "Anthracite" and "Leila," the higher pressure gave least efficiency. We may, perhaps, obtain some idea of the relative efficiencies that should have been attained in the following manner:

Assuming that the steam condensed and re-evaporated had one-fourth the value of that remaining, the work done per unit of weight of working steam becomes for the two cases nearly as $\frac{70 + 7\frac{1}{2}}{70} = 1.11$ is to $\frac{90 + 2\frac{1}{2}}{90} = 1.03$, and as 16 pounds of steam per hour and per horse-power is to 15.9—practically the same, although the steam pressure was twice as great in the first case as in the second. We are evidently finding it more and more necessary to discover some means of making the interior surfaces of our steam cylinders of non-conducting material. That accomplished, the cost of power, in quantity of steam used, will be re-

duced from 10 to 50 and more per cent., according to the kind of engine considered. Until that is done, superheating, steam-jacketing, and high speeds of piston must be relied upon to give high efficiency; but only perfectly adiabatic expansion can give maximum economy of steam.

The error, long since detected by engineers experienced in the management and familiar with the working of steam-engines, which has been fallen into by writers of authority, who have assumed that the condensation of steam due to transmutation of heat into work, discovered by Rankine and Clausius, produces the principal part of the water observed in the cylinders of engines working dry steam, is becoming generally recognized, and later writers are in a fair way to learn that it is not the fact that "the greater part of the liquid water which collects in unjacketed cylinders" is "produced by liquefaction of the steam during its expansion;" but that this latter amount is insignificant, and that this water comes of cylinder condensation, sometimes with considerable leakage, and often amounts to a half or more of all the fluid supplied by the boiler. This fact once well understood, it may be hoped that this defect, existing in all heat engines, may soon be remedied to such an extent as no longer to constitute the great obstacle to further advance. The working of a fluid, of which the efficiency depends upon adiabatic change of volume, within a vessel so perfectly pervious to heat as is an iron steam cylinder, from the physicist's standpoint, involves an absurdity.

The trials of steam-engines, now often conducted by the Farey & Donkin method of measuring the heat rejected, afford a reliable means of measuring actual efficiencies. Recently, Eckart has applied the chronoscope of Hipp to the determination of the exact volocities of piston in mid-stroke, and we may expect soon to know much more than we do at present of the precise action of steam in the engine, and of causes of variation in efficiency.

Naval engineering is one of the most interesting and important branches of our profession, and the progress which has been made in its field during our generation illustrates the advances observed in nearly every other department. Naval works, whether in the civil or the military—in the "merchant" or the distinctively so-called "naval"—marine is to-day become almost purely the work of the mechanical engineer. The shipbuilder constructs his ships of iron and steel; their lines are laid down by the laws of engineering science; their parts are formed in the

machine shops, and put together by the same methods that are adopted in constructing their boilers. They are driven by steam engines designed and built by our fellow-engineers, and the winds no longer either aid to any great degree or seriously impede their progress. Even their loading and the discharge of their cargo have become minor matters of engineering. The old-fashioned mariner is rapidly disappearing and the engineer is likely to become the responsible officer on the voyage as during construction.

Progress, if not more rapid in the navy than in the army, is more observable, and to me, at least, and perhaps partly because of my personal knowledge and closer relations, more interesting in its connection with engineering. A generation ago, the French "Napoleon" line-of-battle ship, with her 100 guns and 600 horse-power engines, represented the most formidable of naval vessels. A little later—1856—our "Wabash class" of screw frigates, with their fewer, but much heavier, guns, were thought the type of the coming fleet; but it was then that the modern ironclad came to revolutionize all naval warfare. Those greatest of engineers, Robert L. Stevens and John Ericsson, and the greatest of naval architects, Edwin J. Reed, have led the way to the construction of the war-ship of to-day—a craft carrying ordnance weighing from 25 to 160 tons, at speeds varying from 12 to 16 knots; plated with from 14 to 30 inches of armor, and yet penetrable by their own guns—a great fighting machine, designed, constructed, and mainly operated by engineers. The daily advance noticeable in naval construction is a progress leading directly and rapidly toward bringing all naval warfare within the province of mechanical engineering. The fighting sailor of earlier days is giving place to the fighting engineer and mechanic, upon whom success in handling these great fighting machines must inevitably depend. To-day, if two professions are to be combined, it is easier for the engineer to learn and to practice the duties of the sailor than for the seaman to make himself at home among the cranks and shafts, the rods and the valves of the engineer. In our own navy, the line officer is becoming a skilled engine-driver, and the engineer is studying in all the higher departments of his profession, both in naval and steam engineering.

But a revolution is impending that will produce, as yet, unknown changes.

Ten years ago, I proposed a classification of naval vessels, which was a little later again proposed by J. Scott Russell in a modified

form. I stated that the increase so rapidly taking place in weight of ordnance and armor must sooner or later compel the division of all navies into three classes of ships, and an independent service of torpedo vessels: (1), A class of vessels for service in time of peace, of moderate size and speed, carrying a few heavy guns, unarmored, and with great sail power; (2), a class of unarmored ships of very high speed under steam and carrying a light battery, such ships as might be best calculated to destroy the commerce of an enemy; and (3), a class for heavy fighting, carrying the heaviest of guns and the most impenetrable of armor, with as high steam-power as possible, and rendered, by division into compartments, as nearly unsinkable as possible. A few years later, I stated that "the introduction of the stationary, the floating, and the automatic classes of torpedoes and of torpedo vessels has now become accomplished, and this element, which it was predicted by Bushnell and by Fulton, three-quarters of a century ago, would at some future time become important in warfare, is now well recognized by all nations. How far it may modify future naval establishments cannot yet be confidently stated, but it seems sufficiently evident that the attack, by any navy, of stationary defences, is now quite a thing of the past. It may be looked upon as exceedingly probable that torpedo ships of very high speed will yet drive all heavily-armored vessels from the ocean, and complete the parallel between the man-in-armor of the Middle Ages and the armored man-of-war of our times." These words are fully justified to-day; and the non-success of naval vessels in later wars, and the production of such craft as the Polyphemus, making 17 knots, and as Ericsson's Destroyer, with its great submarine gun, and the self-propelling torpedo, guided from the shore, are simply very large straws, showing that the coming days of freedom on the high seas and of cessation of all naval warfare are not far away. This most splendid of revolutions is to be the work purely of the mechanical engineer. I have no doubt that many among my audience will live to see that forerunner of the millennium.

Gunnery is a branch of our profession which has been too much neglected by engineers; and progress, dependent upon laymen and upon a few of the military class whose tastes rarely lead in the direction of construction, although, perhaps, much more rapid than should have been expected, is much slower than may be anticipated when this special department becomes the chosen

field of educated, well-trained, and talented mechanical engineers.

From the days of Tartaglia and of Rumford, the direction of movement has been readily traceable. Stronger and safer ordnance metal, breech loading in place of muzzle loading, increased velocity of projectile, a flatter trajectory, with less lateral drift and with enormously increased range, are the features of changes now occurring. Whitworth's compressed steel, Krupp's breech mechanism and skilful design and construction have given us guns capable of driving shot at velocities of over 1200 feet (over 360 meters) per second with small arms, and nearly 2000 feet (600 meters) with heavy ordnance. Whitworth, with a comparatively small piece, has attained a range of nearly ten miles. The "machine guns" of Gardner, as built by Pratt and Whitney, and the Gatling and others, as constructed by the Colt Company and the Ames Manufacturing Company, firing a thousand shots a minute, have rendered the old methods of warfare, in which large masses of troops were deployed in the open, entirely obsolete, while the accuracy of sharp-shooting at ranges of 1000 yards or more makes the use of any unprotected ordnance at short ranges extremely difficult.

Hollow cast guns, as made by Rodman, although the best cast-iron ordnance ever known, are now of the past; and even the Armstrong, the Woolwich, and other guns built up in the forge, fail when made of 80 and 100 tons weight, as now demanded, and must inevitably, as I predicted several years ago, give place to solid steel guns of the Whitworth or other stronger type. Improved methods of making explosives and better adjustment to the work by variation of composition, and especially size and density of grain, has enabled us to keep pressures much below 25 tons per square inch, while greatly increasing the energy developed per pound burned and correspondingly increasing the effectiveness of ordnance.

The theoretical energy of good powder is about 250,000 or 300,000 foot pounds (80,000 to 90,000 kg. m. per kg.) nearly per pound. In experiments, every day in progress, we now get an actual result equal to two-thirds.

This is a branch of thermodynamics that must soon attract the attention of some scholarly engineer, familiar with practical work, and we may hope that so fruitful a field will not much longer be left so entirely uncultivated. There is, to-day, however, no diffi-

culty in designing a gun to do any specified work, and but little in determining the resistance to be met and the energy demanded at a given range, or when attacking armor-plate. Fortunately, however, the days of iron-clads and of naval warfare will soon be numbered. Guns can be made to penetrate any thickness of armor that can be floated, and when the importance of a long bore, with a slow-burning or intermittently burned charges, becomes recognized, the effectiveness of ordnance against any armor will be such that the iron-clad will soon vanish from the seas. There is still much to be done in perfecting ordnance, however, especially in its construction, and as yet our ordnance officers are completely at sea in respect to systems of construction of large guns. Treadwell and Woodbridge have pointed out one direction of progress by the application of the strongest known form of metal—hard-drawn steel wire—in building up the barrel, and Whitworth has shown what wonders can be accomplished with steel in masses. Some of our fellow-engineers will undoubtedly go still further in this direction. The gun is already a heat engine of high efficiency, but thermodynamic investigations will show that this gas-engine may be made still more efficient, and the chemist and the engineer will aid each other in perfecting it. A gun in which the charge expands twenty-five times should give to the shot an energy of 300,000 foot-pounds per pound (90,000 kg. m., nearly) of good ordinary powder, and such a standard must sooner or later be closely approximated. As the heat is generated and expanded in a very small fraction of a second, the gas expands adiabatically, or isentropically as Gibbs and Clausius would say, and the loss should be small except by incomplete expansion. It is sufficiently evident that we are yet to see the air-chamber used intelligently, and, therefore, our guns lengthened greatly, and carefully proportioned to their work. It does not even yet appear to have become understood that recoil is often simply an evil, and an avoidable one, with breech-loading guns; but the time must come, I think, when ordnance, whenever possible, if maximum battering power or range is required, will be held fast against recoil and thus the defect in efficiency, all the inconveniences and some of the dangers now due to this waste of energy will be avoided. Recoil is, with modern ordnance, often an unmitigated and inexcusable evil. Increased accuracy and power with flattened trajectory and reduced drift will come with these improvements, and the last will give much greater

convenience and safety in working, and will aid still more in the effort, which the engineer naturally makes, to unite guns and supporting structure as closely and firmly as possible.

That feature of recent progress in engineering which is to-day attracting most attention and awakening most interest in the minds of the public as well as of the profession, is the introduction of machine-made electricity, and of the electric light, but what seems to me the most important phase of this impending revolution is, I think, not yet generally comprehended. By the ingenuity and skill, the courage and persistence, the energy and enterprise of our brother engineers, Brush and Edison and their coadjutors, it seems certain that the dream of the great author of "The Coming Race" will in part be speedily realized, and that for the occasional mild light of the moon, or the yellow sickly flare of the gas flame, will soon be substituted the less uncertain and always available, and always beautiful and mellow, radiance of the electric flame. This is but a beginning, however. A few months ago one of the most earnest and best workers of all who have been with me, at once, friends and pupils, made a very painstaking investigation of the efficiency of a powerful dynamo-electric machine kindly loaned him from Menlo Park. The mean of several series of tests gave, as a result, an efficiency of between 90 and 95 per cent. That is to say, of all the power transmitted to the machine from the steam-engine driving it, over 90 per cent. appeared on the wire in the form of electrical energy. It follows at once that mechanical power may be transmitted through two such machines, again appearing as mechanical power, with a loss of less than 20 per cent. And it follows from this last fact that the distribution of power by electricity is not unlikely to prove a more important application of this wonderful force than is the electric light.

It is to this inestimably important advance in that field in which the mechanical engineer and the electrician have joined hands, that we owe the probably early success of the electrical railway, that promising scheme of simplifying the problem of transportation on our elevated railways; and it is not unlikely that the rising generation may see the completely successful introduction of this method of distributing power from a central source in our great cities, and even from that mighty reservoir, Niagara, with its 3,000,000 horse-power, to far-distant cities on either side of this great Continent. Sir William Thompson has stated it as probable that 25,000 horse-power may be sent by this method from Niagara to

New York, Philadelphia, or Boston, through a half-inch copper wire, losing twenty per cent. in transmission; he would effect distribution by using the Faure battery as an accumulator. The competition of this method of distributing light, heat and power with the already practical plan of steam distribution introduced by Holly, of Lockport, and now coming into use in New York city under the direction of Emery, will be watched with unusual interest.

I have sometimes said that the world is waiting for the appearance of three great inventors, yet unknown, for whom it has in store honors and emoluments far exceeding all ever yet accorded to any one of their predecessors.

The first is the man who is to show how, by the consumption of coal, we may directly produce electricity, and thus, perhaps, evade that now inevitable and enormous loss that comes of the utilization of energy in all heat engines driven by substances of variable volume. Our electrical engineers have this great step still to take, and are apparently not likely soon to gain the prize that will yet reward some genius yet to be born.

The second of these greatest of inventors is he who will teach us the source of the beautiful soft-beaming light of the firefly and the glow-worm, and will show us how to produce this singular illuminant, and to apply it with success practically and commercially. This wonderful light, free from heat and from consequent loss of energy, is nature's substitute for the crude and extravagantly wasteful lights of which we have, through so many years, been foolishly boasting. The dynamo-electrical engineer has nearly solved this problem. Let us hope that it may be soon fully solved and by one of those among our own colleagues who are now so earnestly working in this field, and that we may all live to see him steal the glow-worm's light, and to see the approaching days of Vril predicted so long ago by Lord Lytton.

The third great genius is the man who is to fulfil Darwin's prophecy, closing the stanza:

> "Soon shall thy arm, unconquered steam, afar
> Drag the slow barge or drive the rapid car,
> Or, on wide-waving wings expanded bear
> The flying chariot through the fields of air."

The quotation may excite a smile to-day, but when first published, just one hundred years ago, the last lines must have seemed hardly more extravagant than the first.

And it is to-day true that we are getting on, that even in the science of aeronautics progress, although slow, is still to be observed year by year, and there is no department of engineering in which the art of the mechanic has opportunity for greater achievement. We have not yet learned to fly like Daedalus, and thus have escaped the fate of Icarus, but the flying automata of Archytas, and of Regiomontanus have been matched in our own times, and the navigation of the air is, very possibly, on the point of real advancement.

When it is considered that it is only ninety-eight years, last June, since the brothers Montgolfier invented the balloon inflated with hot air, and that two months later M. Charles made use of hydrogen for inflation, it will, I am sure, be admitted that the progress which I am about briefly to sketch is far from being discreditable. Since Charles Green, the famous English aeronaut, just sixty years ago, substituted coal gas for hydrogen, the progress of ballooning has been rapid, and science is greatly indebted to Biot and Gay Lussac, to Flammarion, to De Fouville, and especially to Glaisher, among balloonists, although, as yet, little direct advantage has come to mankind from their efforts. The practical application of the balloon has been confined almost exclusively to the purposes of military reconnoissance. During the Franco-German war the great French naval engineer, M. Dupuy de Lôme, succeeded in giving to the balloon a slow motion by means of a screw, and in directing its course by a rudder. His balloon was spindle or cigar-shaped, and contained 12,000 cubic feet of gas. It could carry fourteen men, and the screw was worked by four or eight men. But while it could be moved slowly in calm weather, this machine gave no encouragement to hope that self-impelling balloons will ever become successful. To support the weight of machinery they must have great bulk, and with great bulk no machinery yet devised is light enough, yet strong enough, to drive them at any such speed as is necessary for navigation in even a moderate breeze. Our only hope lies in the direction of flying machines, lifted by their own power, not buoyed up by gas.

And this scheme cannot hastily be condemned, nor by any means at once decried as chimerical, although, to-day, there is but little accomplished by man in this direction. The carrier pigeon and the wild goose are but animated flying-machines, and it can hardly be pronounced impossible that man shall yet compete with them in their own element, as he has long since learned to excel

the fishes in their element. And a little has actually been done. Men of science like Pettigrew, Marey, and De Lucy, have studied the motions of the wings of birds and insects, have learned the laws of fluid resistance, and have paved the way to a real advance. The theory of propulsion has been long studied, and in some directions well established. It has been shown that weight is probably not objectionable in aerial navigation, but actually a necessity; not weight but volume constitutes the impediment. A bird is a heavy but compact structure, of which the essential characteristic is that it incloses great power within small volume. De Lucy's measurements of various flying creatures show an irregular, but still unmistakable, general direction of variation of wing surface with size of animal. Comparing the lady-bird and the stag-beetle, the pigeon and the stork, the sparrow and the crane, we find the area of wing per unit of weight carried to be nearly as the cube root of their weights. Taking as a fair figure that obtained from the larger bird, I find that a man of the ordinary weight should be able to fly with wings having an area of only about 40 square feet (nearly 4 square meters). De Villeneuve states that a bat having the weight of a man would need wings only 10 feet (3 meters nearly) long. Hastings makes the surface of each wing from 5 to 10 times $\frac{2}{3}\sqrt{W}$ where the area is measured in square centimeters and the weight in grams.

Marey has made birds in harness record graphically the motions of their own wings, and Haughton and Marey and others have determined the working power of muscles in proportion to weight and size, and the method of movement of muscles and wing. Henson, Stringfellow, May, and others have made self-impelling model flying machines, some of which have actually lifted themselves in the air, and several of which have flown with great speed when once lifted clear of the ground. But the most remarkable achievement of all, perhaps, is that of Henson in making a steam-engine, fragile to be sure, but still a working machine, producing a third of a horse-power, and weighing *less than* 15 *pounds* ($6\frac{1}{2}$ kilogs). This machine was certainly more powerful than any bird of its weight could be. It is here that we seem most likely to be held in check, and it must be confessed that there is as yet but little on which to base an expectation of finding a satisfactory yet powerful motor. Thus we are apparently approaching, though still, perhaps, far from this goal, and we may barely venture to hope that the engineer who is to

combine the elements of success, all of which are becoming determined, will in our own day win the fame that awaits the first successful builder of a flying-machine.

But all the efforts during this most wonderful of centuries just passing, of either men of science or of engineers, would have been of little avail in the world, would have been unfruitful, however intelligent and however energetic the workers, without that other mighty power which preserves all science and sustains all art, which perpetuates both the fame of the inventor and the knowledge of his inventions. The art of printing, originating in an unknown past, dating its first grand expansion from the time of Guttenberg, and the use of movable types, four centuries ago, has seen its grandest development during this last half century.

The introduction of the power press, and the gradual incorporation into one automatic machine of the web perfecting press of Sir Rowland Hill, and of Jephthah Wilkinson's, of Worm's cylindrical stereotype plates, of Richard Hoe's type-cylinder and double-acting fly-frame, of Applegarth's enlarged impression-cylinder, and of minor improvements, have led to the creation of the modern press.

To-day a daily paper can be printed at the rate of 30,000 impressions an hour, each paper printed on both sides, cut from the great roll—hundreds of yards long—in which it came to the press, pasted in shape and folded exactly to size, and then counted off by the machine as delivered to the carrier. The work of the compositor is soon likely to be wonderfully accelerated by the type-setting machine, which has attained to-day most extraordinary perfection. Paige's machine receives a column of "dead matter" from the press, distributes it automatically, sets it up anew at the rate of 3500 "ems" per hour, including setting, justifying, and distributing,—five times the work of the unaided hand. Its type lasts longer than when set by hand, and every defective or turned type is thrown out by this mechanical automaton.

Of all the observable signs of progress that attract our attention in these stirring times none are more interesting, and none more vitally important, than those which indicate the progress of this nation and of the world in the means and the methods of preparing the coming generation for its work.

The accumulation of wealth depends upon our material progress, and constitutes the only means of securing a steady pro-

gression in civilization, of conferring upon the world the blessings of intellectual and moral advancement, and of comfort and healthful luxury. But the accumulation of wealth means, not the piling of gold and silver in treasury vaults, and not the aggregation of fictitious values in Wall Street, but the production of real property in buildings, in enriched lands, in mill machinery, in means of transportation, and in every form of durable material essential to the creature comforts of mankind.

The accumulation of real property depends largely, if not almost entirely, upon two great social conditions: (1) the cheapening of food and other destructible necessaries of human life by the introduction of labor-saving mechanisms and processes; (2) the steady and skilful application of the intellect and of the manual skill thus set at liberty, to the production of that form of permanent wealth, the accumulation of which during the past century has given to the working-man of to-day comforts and luxuries unknown to kings and princes on the day of the birth of our country.

Now, all that can be done in this direction must be done principally by the mechanic and the engineer, and our immediate duty is to see to it that our children and our children's children shall have every opportunity to acquire that knowledge and that intellectual power, and to gain those means and powers of directing the forces of nature as well as to utilize their own natural strength and skill, and thus to do this work, when the opportunity comes to them, with highest success and most thorough efficiency. It is in providing the opportunity demanded by every good citizen to make his sons and his daughters capable of doing the work so coming to them, that the highest duties of the state remain to be fulfilled, and here it is that the signs of the times are most cheering.

When every man and woman, every boy and girl in the land is guaranteed the privilege of learning any business, and of engaging in any occupation, that he or she chooses, or that circumstances may render advisable, and whenever and wherever it may seem best, a long step in advance will have been taken.

But the individual must be taught, not simply *permitted* to learn as best he can. Education, directed effectively with the object of giving, in least time and at least cost, a preparation for all the duties coming to the learner, whether in daily toil or in social life, is called for; trade schools must be incorporated into

the common school system, and technical and professional colleges and great *Universities of Science and Art* must be placed beside the older academies of learning. And this need is most felt by our own colleagues, and by the people employed by them. He who would accomplish most in the profession of the mechanical engineer, or in the trades, must best combine scientific attainments—and especially experimental knowledge—with mechanical taste and ability, and with a good judgment ripened by large experience. He must be carefully, thoroughly, and skilfully taught the principles of his art in the technical school, and the practice of his profession in office or workshop.

We have been late in seeing this necessity, and must suffer for our dulness as a nation ; but we are beginning to open our eyes and to move in this most vital of all the duties of citizenship. One and two and three centuries ago, wise men like Pascal and Worcester and Vaucanson saw this greatest, highest duty of governments and citizenship, but it is only recently that we, as a people, have come to see its importance.

But now, the magnificent trade and technical school system of Germany, the older if less complete educational system of France, the tardily begun but splendid later work of Great Britain, and the grand beginnings made in the United States, form a glorious commencement of a revolution that shall peacefully effect such changes during the next generation as probably no one can realize until after their actual accomplishment.

With trade schools in every town, technical schools in every city, colleges of science and the arts in every State, and with a great technical university as a centre for the whole system, we shall yet see all combined in a social organization that shall insure to every one absolute freedom to learn and to labor in any department of industry, with absolute certainty of a fitting recompense for all the zeal, intelligence, and good work that the worker, whether man or woman, may offer the world. Then, and then only, will the memory of those greatest benefactors of their race, Case and Hoe, Vassar and Durant, Rensselaer and Rose, Stevens and Packer, Pardee and Washburn, be generally revered as we to-day revere them.

Then, and then only, will our profession attain its noblest development, and its science and its art, in closest union, join most gloriously in the great work of emancipating mankind from the trammels of this animal nature, and of relieving the race from

the pressure now felt in the terrible struggle for the necessities of life, substituting arms of iron and fingers of steel for these weak members of flesh to do the work of the world, leaving every human being having brains the opportunity to acquire knowledge, to enjoy life, liberty, and the pursuit of happiness, and to prepare for the future, as the knowledge, judgment, and faith of the individual may dictate.

LI.

AN IMPROVED METHOD OF SCREW PROPULSION.

BY JOHN B. ROOT, NEW YORK CITY.

In the summer of 1879 I determined to build a small pleasure-boat for my own use, with the view of demonstrating the value of the double hull principle of construction, which had to my mind advantages, as to the room, stability, etc.

I built a boat having two hulls, 30 feet long, placed 8 feet apart in the clear, and decked over from hull to hull.

The propelling machinery was placed in the centre ; the engine crank revolved in a horizontal plane; the shaft going down through the deck, and driving a horizontal propeller-shaft by means of mitre gears working below the water.

The boat answered my expectations, but the noise produced by the gears was annoying, and I decided to dispense with them, and by inclining the shaft downward and backward to work the screw direct from the engine.

I expected a loss of economy in so doing, but preferred that to the noise.

In carrying out the change, I began to figure up the loss that would be encountered, and was surprised to find that I could not only obviate the loss, but as I then thought could realize a positive gain.

This led to a series of experiments during the last two years, which, by their results, have convinced me of the entire soundness of my theory.

My object in writing this paper is to promulgate what I consider to be valuable discoveries in the art of screw propulsion, and to induce others to invest money in a more thorough experi-

mental demonstration of the principles involved, and their application to useful purposes.

I have not attempted to give exact figures, but have endeavored to set forth the principles involved, and their mode of application in producing the results I have obtained.

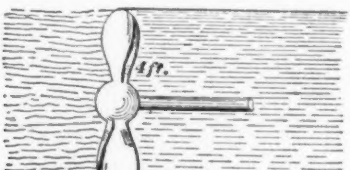
I claim to have discovered an improved method of applying screw propellers to vessels with the following advantages:

1. Obtaining the same or greater effect upon the vessel by the use of a very much less angle of blade or pitch, by which a great part of the loss from side action or slip is obviated and the propelling power economized.

2. The resistance of the vessel is diminished by the lifting action of the screw, lessening the displacement.

To get a clear understanding of the advantages of my improvement, it is necessary to know how great is the loss by the present method and how it occurs. The essential points are:

FIG. 136.



1. The requisite thrust upon the vessel.

2. The power required to produce the thrust.

Fig. 136 shows a screw of an ordinary form, with a pitch of one and one-half diameters, and a horizontal axis, which, by its action, pushes the water directly astern, causing a forward thrust upon the vessel.

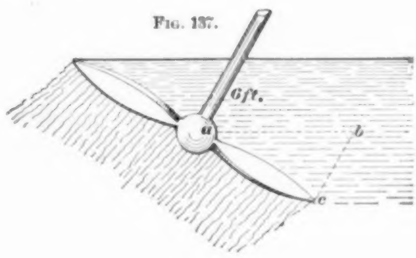
Fig. 137 shows a screw having about one-half the pitch of Fig. 136, and double the area of disk, with its axis inclined at an angle of 60° from the horizon, and acting to propel the vessel by pushing the water downward and backward.

The thrust of the screw being at an angle of 60° from the line of movement of the vessel, there is required double the force of thrust to produce the same forward push upon the vessel that would be required if exerted in a line with the movement of the vessel. This increase of thrust is provided by enlarging the

diameter of the screw, until its disk area is sufficient to give the required thrust without excessive slip, when it will give the same forward push upon the vessel as Fig. 136, which has double the pitch and one-half the disk area.

I will compare the operation and effect of the two methods, and, in doing so, will assume that a screw by its action sends through its disk, in a line with its axis, a stream of water of the same diameter as the screw, at a velocity equal to the pitch of the screw less the velocity of the vessel. That is, at each revolution, the water will be moved a distance equal to the difference between the pitch of the screw and the velocity of approach of the water to the screw. Whether this is strictly true or not will make no difference for the purposes of comparison. In comparing the operation of the screws shown in Figs. 136 and 137, it is intended that they shall propel the vessel the same distance with the same

FIG. 137.



number of revolutions; Fig. 137 giving double the thrust of Fig. 136, with the same or less power applied.

In Fig. 136 the motion of the water through the screw disk is in the same direction that the pitch of the blades act. The blades are acting upon water that has entered the screw with a velocity less than that with which it is discharged from the screw astern.

To illustrate, we will suppose a vessel with screw Fig. 136, to be going through the water at a velocity of 10 feet per second, and that the velocity of pitch is 12 feet per second; it is evident that there would be imparted a velocity to the water of 2 feet per second. The difference between the velocity of approach (10 feet) and the velocity of pitch (12 feet) being 2 feet per second, this is the motion given to the water astern.

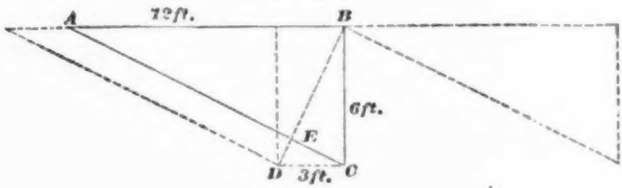
This velocity of 2 feet per second is given to the water as it leaves the screw disk.

The after side of the disk pushes the water back, and the water

in front follows to take its place in exactly the same manner that the water follows the piston of a pump. The pressure is enough greater on the after side of the disk than on the forward side, to furnish the requisite force, to give the water passing through the screw its velocity.

As the screw revolves each blade impinges upon the water that has been put in motion by the preceding blade. If the velocity at the point of impingement given to it by the suction of the

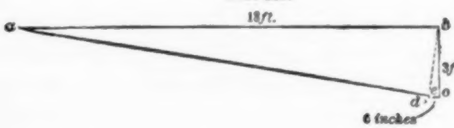
FIG. 138.



preceding blade were 1 foot per second, there would remain only 1 foot per second to be imparted to produce the velocity of 2 feet per second; 1 foot per second being imparted by the screw and the same due to momentum acquired before entering the screw.

If the screw were moved sidewise through the water in a line at right angles to its axis, the blades would strike or impinge upon water that had not been put in motion by the preceding blade, and there would be more velocity to be imparted by each blade, and consequently a greater thrust.

FIG. 139.



To bring out this point more clearly, we will suppose a screw were working in a circular channel, as shown in Fig. 143. The water would be put in motion around the circle and would return to the screw with the same velocity with which it left it. After the *inertia* of the water had been overcome, there would be no power required to keep it in motion, except to overcome the friction of the channel, and there would be no thrust given by the screw except that required to overcome the friction, and the veloc-

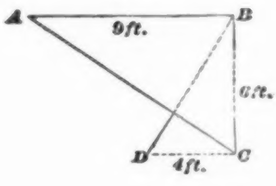
ity would remain constant from the first starting of the screw. But, in open water, the case is different; the velocity given to the water is dissipated in the mass of surrounding water, and the screw has constantly to put new water in motion from a state of rest, or increase the velocity of that which is in motion; hence the pressure of the screw, and its thrust upon the vessel.

It is only as it gives or adds motion to the water passing through it, that the screw gets the requisite resistance to propel the vessel.

It is necessary that each blade, as it revolves, should strike far enough ahead of the preceding one to get hold of water that has less than the maximum or pitch velocity.

Fig. 144 is intended to show the increase of the velocity as the water approaches the disk, caused by the suction of the screw, and its decrease after it leaves it. The suction of the screw will bring the water towards it in a convergent stream, the velocity of which

Fig. 140.



will increase as its area diminishes, until it reaches the screw disk, when it will have its maximum velocity, as it is discharged from the after edge of the blade. The leading edge of the next blade will have its path far enough ahead of its predecessor to get hold of water that has less than the maximum velocity.

If, without changing the pitch, there was substituted for an ordinary two-bladed screw one having a great number of very narrow thin blades, the thrust would be found to be very greatly diminished, for the reason that the difference in velocity of the water between entering and leaving the blades would be very slight, and yet the water would leave the screw at the same velocity as with the two-bladed screw.

The result would be similar to that of the circular channel (Fig. 143), inasmuch as the blades would act only upon water having nearly its maximum velocity, *which once imparted by the first starting of the screw would remain constant*, so long as sufficient power was employed to overcome the friction, or the molecular

resistance of the water. The water leaving the screw disk with its maximum velocity, by its momentum draws other water through the disk from the front, to take its place.

I am not aware that this peculiar action of the screw propeller is fully understood.

The rules for proportioning the pitch to the number of blades given in Mr. Burgh's book, would indicate that it was not understood. The rule given is in substance this:

"The more blades the less pitch, or two blades one and one-half diameters, three blades one and one-third diameters, four blades one diameter."

Now as either reducing the pitch or increasing the number of blades causes them to strike or impinge upon the water nearer the point of maximum velocity, it seems to me that this rule is essentially wrong and shows that the matter was not understood.

I am not aware that any explanation or allusion to it has before been published. The matter was forced upon my mind by unlooked-for experimental results which led to further investigation.

The pressure of the screw disk upon the water, and its consequent thrust upon the vessel is, according to Rankine, "the product of three factors: the mass of a cubic foot of water; the number of cubic feet of water acted on in a second; and the velocity in feet per second impressed upon the water by that propeller."

The mass of a cubic foot of water being taken as 2, being the weight of a cubic foot of sea-water (64 pounds) divided by the accelerating effect of gravity in a second (32 feet).

For instance, if the vessel has a speed of 10 feet per second, and the pitch velocity was 12 feet per second, the area of the screw disk being 12 feet, the reaction or thrust would be found thus:

(Mass 2) × (No. feet acted on **144**) × (velocity impressed upon that water 2 feet per second) = 576 pounds, but if, as I believe, a part of the velocity impressed upon the water by the screw is constant from the first starting of the screw, then the thrust of 576 pounds is too great.

The "pitch velocity" may be resolved into its two components, the direct velocity and the lateral or transverse velocity.

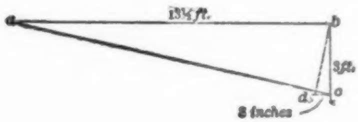
The power applied to the vessel by the screw is the thrust × by the direct velocity. The power that is required in excess of that, shown by multiplying the thrust by the direct velocity, is a loss, and comes from three causes.

1. The thickness of the blades offer resistance.
2. The surface of the blades gives frictional resistance.
3. The pitch or angle of the blades causes the water acted upon by them to be moved or discharged from the screw disk sidewise or at an angle to the axis, instead of in a line with, or parallel to, the axis of the screw.

It is necessary in considering the action of the screw propeller, to discard the idea of a screw turning in a solid nut, which is a positive, mechanical movement, the turning of the screw giving in all cases an exact equivalent of progressive motion. But a screw working in water is an entirely different thing, and the only way to estimate its reactionary effect in any one direction is to compute the weight of water moved and its velocity in an opposite direction.

A certain reactionary force can be got more economically by moving a large amount of water at a low velocity than by a

Fig. 141.



small amount at a high velocity, if the resistance of thickness and surface of blade is not increased sufficiently to counteract the gain.

By applying the foregoing principles to the action of the inclined screw, shown in Fig. 137, its comparative effect can be estimated. In Fig. 137 it will be observed that the water enters the screw, as the vessel moves ahead, at an angle of 60° from the axial line of the screw, and not parallel with the axis, as in Fig. 136; and, consequently, when the screw and the vessel are moving through the water at the same velocity as in Fig. 136, the water entering the screw approaches it with only one-half the velocity. By reference to Fig. 137, it will be seen that the vessel and screw must move the distance from *a* to *b*, while the water approaches towards the screw the distance from *b* to *c*.

The velocity of approach of the water towards the screw is only one-half that with which the screw passes through the water as the vessel goes ahead. The result necessarily follows that the blades of the screw act constantly upon water that, when it

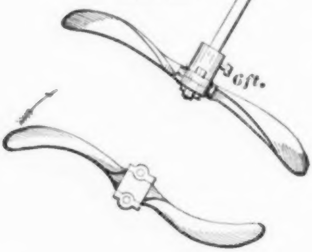
enters the influence of the screw, has only half the velocity of approach that it would have if it entered on a line parallel with the axis of the screw.

For instance, as in the former case, the vessel is moving through the water 10 feet per second, the velocity of approach is 5 feet, the velocity of pitch 6 feet, leaving a velocity of 1 foot per second to be imparted to the water. But, as the screw progresses partly sidewise through the water, the blades are acting upon water that has not been put in motion towards the screw by the action of the preceding blades. This leaves a greater proportional velocity to be imparted by the blades to the water, even though its velocity on leaving the screw disk be less.

Figs. 145 and 146 show the path of the blades of the inclined screw through the water.

It will be seen that the path looked at on a line parallel with

FIG. 142.



the axis is cycloidal. I therefore have named it "the cycloidal screw propeller."

Let us now determine the thrust of the cycloidal screw by the same method as with the horizontal screw, Fig. 136, the number of turns being the same in both cases. The area being 24 square feet, the pitch velocity 6 feet per second, $24 \times 6 = 144$ cubic feet, the number of feet passing through the disk in a second; the reaction or thrust is $2 \times 144 \times 1 = 288$, only one-half that of Fig. 136, if there be no allowance made for the constant velocity of the water in Fig. 136. *Yet in my experiments it was demonstrated beyond all doubt that the incline screw gave as great a speed upon the boat with less power.*

To maintain the same forward push upon the boat, the thrust of the inclined screw should be double that of the horizontal, when by the same rule it is only one-half.

I account for the result by the constant velocity of the water in

the horizontal screw diminishing its thrust, and also by the inclined screw putting in motion a larger body of water than passes through it, increasing its thrust.

The transverse area of the stream of water that is put in motion in one second by a cycloidal screw is greater than the area of the screw. If the screw were about five and one-half feet in diameter, three feet pitch, and moved ten feet forward, making two turns in one second, it would put a stream of water in motion whose cross-section would approximate seventy-two square feet.

The number of cubic feet acted on would be $72 \times 6 = 432$; the thrust would be determined by (mass 2) $\times$ 432 (number cubic feet) $\times$ 1 (velocity) $= 864$ pounds.

The power applied to the screw in the two cases is the same.

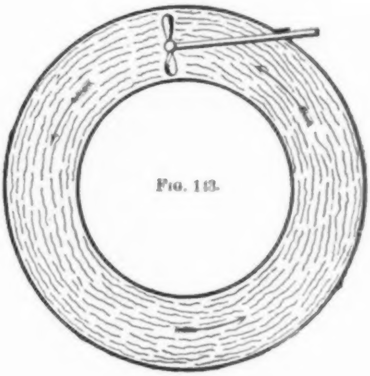


FIG. 143.

In Fig. 136 it is the pressure upon the screw disk multiplied by the pitch.

In Fig. 137, it is double the pressure, multiplied by one-half the amount of pitch, which gives the same result in both cases.

I will now consider the comparative economy of power in the two cases.

The diagrams 138, 139, 140, and 141 are intended to represent the action of the two screws during one revolution, by the equivalent lateral or transverse movement of an incline or wedge. The dotted line $B\ D$, shows the angle at which the water is moved backward by the screw, the reaction being in the direction $D\ B$, the other sides $D\ C$ and $C\ B$, of the right-angled triangle $D\ C\ B$, show the side or lateral reaction in the direction $D\ C$, and the forward or propelling reaction in the direction $C\ B$. If

the force of the reaction be taken as the square of the velocity, it follows, $DC^2 + CB^2 = DB^2$.

The reaction of the stream thrown back by a screw, if the vessel be fast to the dock, is as the square of the velocity of the stream.

If the vessel be under way it is not the same, but must be computed by Rankine's method, which is fully as favorable to the cycloida screw. I have, however, computed the economy on the basis of the reaction being as the square of the velocity, for the reason that without further experimental data it is hard to get at the number of feet acted on by the cycloidal screw, as it puts in motion a very much larger body of water than passes through its disk.

In Fig. 136 we have a screw with a pitch of one and one-half diameters, or the pitch is equal to one-half of the circumference.

Fig. 138 is a diagram of it, in which *A B* is the circumference, *B C* the pitch, and *A C* the angle of blade at its outer end.

The dotted lines at left and right hands of the diagram represent the commencement and end of a movement from right to left, that would move a particle of water from *B* to *D* at right angles to the surface of the blade. The particle would be pushed astern, the distance from *B* to *C*, and would be moved sidewise from *C* to *D*.

It is evident that the movement sidewise from *C* to *D* is a loss, and that the screw must make one and one-fourth turns, to move the particle astern the distance from *B* to *C*, which is a loss of 20 per cent. of the power by side action. The movement of the outer end of the blades is represented by *A B* and *C D*. *A B* being one revolution, *C D* the excess of one revolution necessary to overcome the side action.

Fig. 139 is a diagram of the cycloidal screw of Fig. 137; the diameter is made just one-half greater than Fig. 136.

To avoid fractions, the area of disk being somewhat more than double.

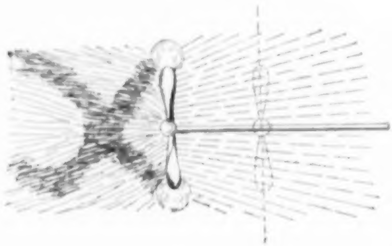
The loss by side action at the ends of the blades in this case is only 2.7 per cent. The loss in both cases at the ends of the blades is very much less than in any other point, therefore we must take a point between the centre and ends of the blades, to get a fair average of the comparative loss by side action in both cases.

I take that point to be one-fourth the length of the blade from the end which gives the loss.

31 per cent. on Fig. 136, and
05 " " 137.

Figs. 140 and 141 are the diagrams showing the side action at one-fourth the length of the blade from the end. If 100 horse-power were applied to driving the screw in each case, the amount of power that would be utilized in propelling effect in each case would be, horizontal screw 100 − 31 = 69, cycloidal screw 100 − 5 = 95, being 37 per cent. in favor of the cycloidal screw. It may be asked, why not diminish the loss from side action in the horizontal screw by diminishing the pitch? Simply for the reason that the number of turns would have to be increased far more than the pitch decreased. If the pitch were decreased one-half, there would have to be given three or more times as many turns to maintain the same force of thrust, on account of the constant velocity of the water, as I have explained. As the loss from friction and thickness of blades increases as the square of the velocity, the loss from

FIG. 144.



these causes would be increased nine times, which would more than neutralize the gain from diminution of side action.

In the foregoing, I have not considered the change in the action, at different points of their revolution, of the blades of the screw, caused by the incline of the axis.

If they are attached rigidly to the shaft, there is a difference in the action of the blades, when they are across or at right angles to the line of motion of the vessel.

When they are moving forward and downward they act with more pitch than when they are moving backward and upward.

To avoid this unequal action, I attach the blades to the shaft by a cross-bearing, or hinge, that causes the action of the blades to be automatically equalized and entirely overcomes any loss by the unequal action.

Fig. 142 shows the construction of screw with equalizing blades. The gain by the lifting thrust of the inclined screw is considerable.

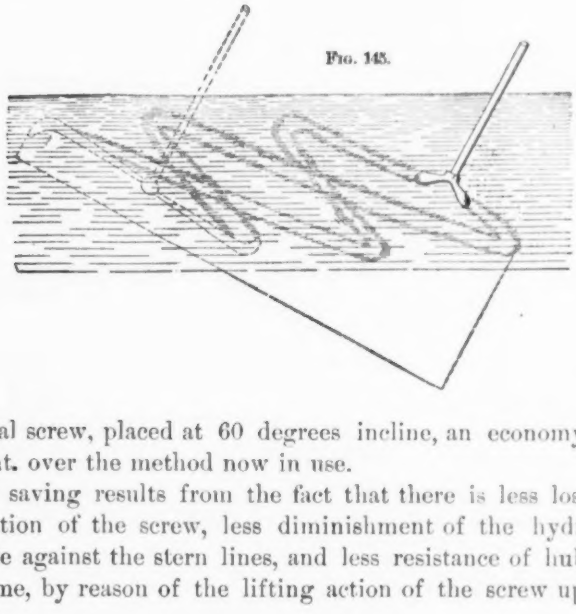
Upon the small boat with which I have made my experiments, I make it nearly equal to one-tenth of the displacement; with large vessels it would not be as much.

The frictional loss of the two methods of applying the screw is to be considered.

In Fig. 137, the inclined screw having about one-half more diameter, would have more friction than the horizontal screw of Fig. 136 if the surface of the blades was increased as the diameter. But the pitch being only one-half, the blades may be only one-half the width or less, which will make the friction in the two cases very nearly the same.

I have come to the conclusion, based upon the foregoing comparisons, verified by experiments, that I can safely claim for the

FIG. 145.



cycloidal screw, placed at 60 degrees incline, an economy of 30 per cent. over the method now in use.

This saving results from the fact that there is less loss from side action of the screw, less diminishment of the hydrostatic pressure against the stern lines, and less resistance of hull to be overcome, by reason of the lifting action of the screw upon the vessel.

If, however, there be no losses from the above causes there would still be loss of power by the screw, as compared with that required to give the same speed if the vessel were towed, and only the power applied to the tow-line considered. I believe that it is conceded by eminent engineers that the loss by the present system of screw propulsion is between 50 and 60 per cent. of the power.

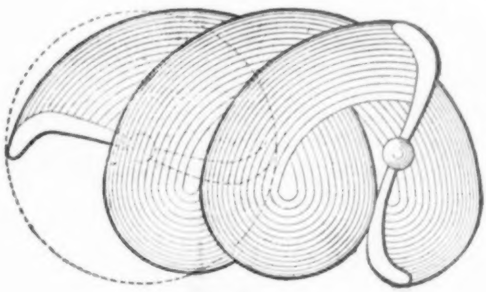
It is my opinion that the power required to propel a vessel a given distance at a given rate of speed, by either of the following methods, would approximate,

Tow-line,	1
Cycloidal screw,	1.33
Horizontal screw,	2

With a screw of only 14" pitch, with axis at 60° incline, 180 *revolutions propelled the boat* 360 *feet, the travel of the pitch of the screw being only* 210 *feet.*

Here is a solution of the negative slip problem. In Burgh's book on the screw propeller, there are numerous cases given where the speed of the ship was greater than the progression of the pitch of the screw.

FIG. 146.



To my mind the cause is the rising and converging of the water at the stern, causing the water to enter the screw at an angle to its axis.

This diminishes the velocity of approach, and there is more velocity to be imparted to the water by the screw.

It will be obvious that the cycloidal screw can be applied by inclining it sidewise instead of downward.

In the foregoing I have confined myself to an angle of 60° for the incline of the axis of the screw, but it is evident any angle of incline will be subject to the same principles of action. The greater the angle of incline, the greater the theoretical gain. The practical limit is the angle at which the increase in the size of the screw would increase the friction, so as to offset the gain from the less angle of the blade.

I quote a communication from the eminent engineer, Charles

E. Emery, that fully elucidates the principle upon which the thrust of the inclined screw operates to propel the vessel :

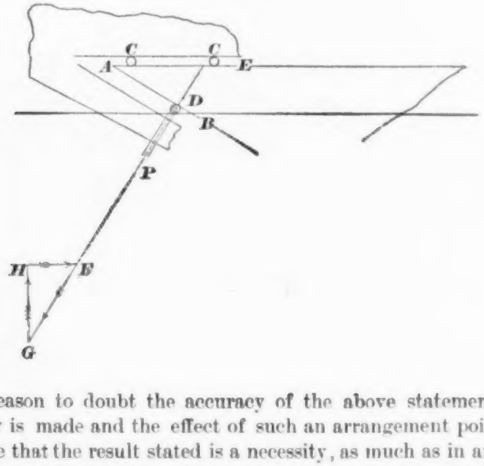
ENGINEERING OFFICE OF CHARLES E. EMERY, 16 COURTLAND STREET,
NEW YORK, February 24th, 1881.

JOHN B. ROOT, ESQ., 28 Cliff Street, New York

DEAR SIR : I am in receipt of yours of the 21st inst. asking, in substance, my opinion of the following statement of your pending application for patent on the subject of screw propulsion.

You say, "I have discovered that by giving the shaft of a screw propeller a sufficient inclination in a direction downward and backward to the line of movement of the vessel, and by increasing the diametric or propelling area of the screw, relatively to the inclination of the shaft, the vessel can be propelled very much further by a given number of revolutions of the screw, than the pitch of the screw multiplied by the number of its revolutions would call for, provided the shaft was placed nearly parallel to the line of the keel as usually applied."

FIG. 147.



I see no reason to doubt the accuracy of the above statement. Now that the discovery is made and the effect of such an arrangement pointed out, it is evident to me that the result stated is a necessity, as much as in any machine for increasing speed where a given force at a given speed is converted into a less force at a higher speed. The result would, however, not be obvious to those infrequently occupied with such problems, so a number of illustrations will be in point.

We force a wedge in a piece of timber, and the wedge moves inward faster than the crack is opened, so the operating force moving through a certain distance produces a greater force moving through a less distance. Now, if the timber be green and tough, the crack may close through the less distance and exclude the wedge, causing it to move further than the sides of the crack move toward each other. So, in any case, a force acting on an inclined surface may move the same at an angle faster than the force itself moves. For instance, it is easy to see, in the sketch, that if a force be applied in the line *P D*, against one incline of a wedge *E A B*, and the face *A E* be guided by rollers *C C*, running against a stationary support, that the wedge will be forced to the right, and that the motion

to the right will be faster than the motion in line PD, when the angle EAB is less than a right angle. By considering that this simple diagram is attached to the stern of a vessel, it illustrates your discovery. The propeller, thrusting downward and backward in the direction DP, produces a corresponding reacting force in the direction PD, which force has two components, one a lifting force, which actually lifts the hull of the vessel a little out of the water, so that the weight thus supported obviates the necessity of the rollers CC; the other, a propelling force to the right, less in intensity than the original force but moving through a greater distance—since the force which acts on the line PD moves only a distance represented by the pitch of the screw multiplied by the revolutions in the time considered. The intensities of the several forces are represented by the sides of the triangle FGH, and the directions of the same by the arrows; the action of the screw moving the water in the direction FG, with a force FG, producing a lifting force GH, in the direction GH, and the horizontal propelling force HF, in the direction HF. Motion of the hull upward, in the direction GH, ceases as soon as the displacement is reduced sufficiently to balance the lifting force—part of the weight of the hull being continuously carried on the screw—hence all subsequent motion takes place in the direction of the line HF, and in order to make the work done, excluding friction, the same in the direction HF as in FG, the motion in the direction HF must be as much greater than that in the direction of the line FG, as FG is greater than HF, a necessity which follows the general law of virtual velocities.

The thrust of your inclined screw being greater than that of one with horizontal shaft, your position is right—that a larger diameter will be necessary to maintain the same rate of slip, and the pitch may also be reduced if desired to reduce the size of the propelling engines.

It will now be seen that the principle is the same as that of a wind blowing angularly on the sail of an ice-boat, where the speed of the boat is not limited to that of the wind.

The conception is novel to me, and I not only believe it to be founded on sound principles, but capable of development so as to be of great practical utility.

Very truly yours,

CHARLES E. EMERY.

NOTE.—I conceive that the mere statement by me as a fact that *the boat has run over* 70 *per cent. ahead of the pitch of her screw, and has shown a saving of power, is sufficiently remarkable to require an explanation of the principles involved, that will show the possibility of such a result, without loading the subject down with the details of experiments, to show the exact saving.* My experience in placing this subject before engineers has been somewhat peculiar. After satisfying myself of the correctness of principle, novelty, and usefulness of the discovery, I determined to apply for a patent. The examiners and various experts of the Patent Office that my attorney consulted denied that my premises were correct. Many other engineers who were supposed to understand the subject were consulted by myself and my attorney; without an exception they pronounced against the correctness of

my premises. I then consulted Charles E. Emery. The plain, straightforward letter given herewith was the result. I then wrote the above paper and submitted it to a large number of prominent engineers. With one or two exceptions I could not find that it received any approval. I then submitted it (together with Mr. Emery's letter) to Professor R. H. Thurston, and received the following letter from him :

STEVENS INSTITUTE OF TECHNOLOGY, DEPARTMENT OF ENGINEERING,
HOBOKEN, N. J., July 21st, 1881.

MY DEAR MR. ROOT : I have to apologize for keeping this paper so long, but I was unexpectedly called out of town, and have had no time to read it as I wished. I leave again this P.M.

It seems to me that a large part of the advantage gained in your experiments must be due to having an inefficient screw to start with, and with which to compare the inclined screw.

The margin for gain on a well-proportioned screw, doing work as it ought, is so small, and the gain possible by more direct effort seems to me, as I look at it now, but without complete investigation, so little, that I am inclined to think that, as between a well-arranged horizontal shaft and its screw, and an inclined screw at the angle of highest efficiency, there cannot be much to choose. Still I am not inclined to be positive, for I have not been able to form a decided opinion relative to the commercial value of the change.

I should like immensely to see it well determined by experiment.

I shall hope to come back to this thing when time permits me to study it.

Yours truly,
R. H. THURSTON.

I am glad to say that it has received the approval of some whose opinions I value. Until it is shown that I am right or wrong in my calculations as to the feasibility of the results I claim to have obtained, the exact economy of results is not material. I invite the severest criticism as to the correctness of my calculations and explanations of these results.

It seems to me certain that the efficiency of a screw should be calculated just as easily and as accurately, having the diameter, pitch, slip, and speed given, as a steam-engine, having diameter, stroke, point of cut-off, and speed given.

If I had made a discovery in steam-engines that would save 30 per cent., it would not be sufficient for me to say I had done it. Even if I gave the exact data of my experiments, engineers would want to know *why*. If, for instance, I had discovered a non-conducting material for cylinders, saving all cylinder condensation, that would account for it. So, in my paper, I account for the sav-

ing by my discovery of a way of obviating the side or lateral action of the screw.

If, as Professor Thurston seems to think, the economy of the cycloidal screw is equal to the horizontal and no more; if my results were deceptive as to economy, it still has great value, as it enables screws to be applied to the sides or any part of a vessel below the water-line, and dispenses with long screw shafts.

DISCUSSION.

Mr. Wolff: The question of the effect of a screw-propeller upon water is one upon which no definite theory has been based, or as far as theory has been developed, it can scarcely be ranked above hypothesis, since certain assumptions on which it is based have not been proved experimentally. It will probably be some time before any theory that can be framed will be able to be fully proved, because there are so many conditions entering the question of screw propulsion. Mr. Root informs us that by his method of inclining the shaft he has obtained results which are superior to those obtained by the use of the ordinary form of screw. The fact that he has obtained these results by experiment entitles them at once to some consideration. Whether the theory that he has framed is correct or not, is a question which I think cannot be answered properly in detail here, barring out the consideration of obtaining higher speed of vessel than the pitch of the screw into the number of revolutions, which is an impossibility under the conditions of the problem of the motion of a vessel through water. It will probably require considerable investigation in connection with the work that has been done by other writers on the subject, to see whether there are any errors in it or not. It would be difficult to do so directly after listening to a hasty reading.

On the question relative to the motion which the screw gives to the water, which is the basis of Mr. Root's hypothesis as to the efficiency of the screw obtained, I would like to refer to the upward action of the screw which takes place in the line of shaft. Mr. Root has referred to that action as adding to the efficiency in raising the vessel and thus decreasing the displacement. I can conceive that in a vessel of very short length this action would take place; but in a long vessel it seems to me the effect of this upward action would be to raise the stern of the vessel and lower the bow, the centre of gravity of the vessel acting as a fulcrum for

the moment of the applied force at the stern of the vessel. If this action would take place, practically the effect would be disastrous to the efficiency which Mr. Root claims to have obtained. Mr. Root's method of propulsion might still be more efficient, in case his theory of impact of the propeller on the water were correct; but certainly the action I have pointed out would detract from its value. This action would not take place if it were considered that the vessel were lifted bodily; but when we consider the length of vessel, say 300 to 500 feet, I think we shall have to concede that the effect of this force, applied at an angle say of 60°, lifting the stern, would have the effect of lowering the bow proportionally—either exactly the same, or to a certain extent—which would make the general effect worse than if there was no difference in displacement between the old and the proposed method of screw propulsion.

Relative to Mr. Emery's remarks in the letter quoted by Mr. Root, as to the motion of the wedge, certainly the motion of the weight might be a greater distance longitudinally or horizontally than the motion of the wedge applied at an angle; but the weight displaced by this wedge with the velocity with which it is displaced, could under no circumstances be greater than the force into the velocity with which it is applied to the wedge. Therefore while the motion of that piece which is moved by the wedge horizontally might be greater than the motion of the wedge, the total amount of momentum given out could under no consideration be greater than the momentum applied. Therefore the illustration which Mr. Emery has taken does not at all substantiate any statement that the actual speed of the vessel would be greater than the speed of the screw, because in that case we have the power into the speed of the screw, as one of the forces. Now if the speed of the vessel becomes greater, it can only become so by increasing the force applied; and since the force applied is a constant quantity in this case, the speed of the vessel could not be greater than the speed of the screw. Even if the speed of the vessel were assumed to be as possibly greater than the pitch of the screw into its revolutions, this would be no evidence that the inclined propeller was more efficient than the horizontal propeller, since the same energy applied to the horizontal screw would not waste a component of energy at right angles to the direction of motion of the vessel, as in the case of the inclined propeller, and for a given coal consumption the performance of the vessel

would therefore *prima facie* be better with the horizontal screw, than with the inclined screw, independent of the relation of velocity of vessel to propeller.

Relative to the question of the impulse of the wind upon an ice-boat, the reason that the ice-boat goes faster than the velocity of the wind, is the following: The component of the wind pressure at any time in the direction of the propulsion of the ice-boat is less than the velocity of the wind, but the effect is cumulative. The wind strikes at an angle and causes a motion horizontally, which it retains. Now the next line of wind strikes upon that, and so the cumulative effect of these different impinging strata or planes of air upon the ship causes the ship to go at a faster rate than the wind itself. We cannot get more momentum out of a thing than we put into it. We may get less actual useful result, because a certain amount of energy may be consumed in overcoming friction, but we cannot obtain more. If Mr. Root tells us that he has obtained a greater speed than the speed of the screw, of course we do not know how far the experiments were correct, but they certainly call for consideration and investigation. All I wanted to point out was that Mr. Emery's illustration would not account for the action taking place which Mr. Root described in the case of a vessel's passage through water.

Mr. Root: I am glad the gentleman has made those remarks. They are in the line of a great many other comments upon the subject,—pronouncing the thing impossible. Now the paper that I have read I suppose will be published and will be brought to the attention of engineers of the highest ability in this Society, and also in the world, I may say. I am quite willing that it shall stand or fall upon its merits. If there is anything wrong in the theory, anything that does not conform to the actual results that can be obtained by experiment, I do not want it to pass. As to one other point the gentleman spoke of—the raising of the stern of the boat —all know, who have given any attention to the subject, that a boat when it is going through the water tends to rise out of the water at the bow. As you force the boat along, there is a natural tendency to raise up and skim over the surface; and it is only a question of giving it enough speed, as I heard Professor Thurston once remark, to cause it to make a mere chalk-line on the water. I recall one instance of a boat where there was an immense amount of power put into a very small compass, and that boat ran out of water one third of her whole length when she was being driven

at a speed of twenty odd miles an hour, just dragging her stern deep enough in the water to allow her wheel to get hold. The same thing occurred with a boat built by Mr. Waldron here, which I believe was on exhibition at Philadelphia during the Centennial. The boat was of a very fine model and there was a great amount of power in it, and that would sink down at the stern—so I understand from those who operated it—so that you could not see a person sitting in the stern of the boat from a short distance. It seems to me that this lifting action of the stern is the very thing you want. The trouble is to keep the boat up at the stern. There is another great advantage if this thing should ever be brought into use, and that is in the length of the shaft between the engine and the screw. At present on all our large ships there is a shaft alley running from the engine back to the screw, in some instances two or three hundred feet long. The alley has to be kept open under the cargo to get at the bearings of the shaft. When the ship is at sea she is swinging and twisting. Two or three of our largest ships, in the past year, have had their shafts broken. The Persian Monarch broke her shaft not long ago. It seems to me that that is a weak point in most vessels. Now with this application you place the engine right over the deadwood in the stern of the ship, and put the wheel right where it is now—not near the bottom of the vessel. A steel shaft twenty feet long, the engine on one end and the propeller on the other, does the business. It is very different from three or four or half a dozen lengths of this enormous shaft two or three hundred feet long going through a channel under the cargo. That I consider to be a very great advantage. As to other points, there is a great deal of course in regard to the application of the power, and the modelling of the ship, etc., that calls for a change. I believe the thing to be right, and I offer it for the criticism that any one has a mind to apply to it, so long as it is kept within lines that can be demonstrated.

Mr. C. T. Porter: For one, I am very much disappointed with this paper of Mr. Root's; very much disappointed indeed. It is a very different paper from that which I expected to listen to from the short conversation I had with Mr. Root yesterday. An inventor comes before this body claiming to have made a discovery which is of a totally revolutionary character. He writes a long paper on the subject and he does not give us one solitary fact—not one. Now what have we got to talk about? Nothing. I do not think that in the absence of facts we have anything what-

ever to consider. If Mr. Root had said that he had attained such and such results, giving a comparison of the speed given to a certain boat by a horizontal propeller with a certain expenditure of power, and the speed given to the same boat with an inclined propeller with a certain expenditure of power, stating the precise conditions under which those experiments were conducted, that would be something. But what have we got? Nothing at all; except that Mr. Emery has given an opinion, and that is the only fact stated. There are several principles supposed to be established in respect to screw propulsion, which I understand Mr. Root to deny, and on that point we have his authority. But as for facts, I could not discover one.

Mr. J. B. Root: I do not think the gentleman could have heard the paper read. I think he must have come in since. I stated as a fact that the speed of the boat was greater than the pitch of the screw multiplied by the number of its revolutions, and I believe I gave the comparison on the paper. I also stated as a fact that there was such a thing as the constant velocity of water through the disk of the screw propeller that reduced the amount of thrust as heretofore computed, and from those two things I went on to account for the results produced and to prove them mathematically; that is to demonstrate them to the best of my ability, though, perhaps, not sufficiently to convince gentlemen. I do not care anything about that. But I advance those things as facts, and I advance it as a fact that the experiments were made and the results produced. Not only that, but I can verify it by proof. I am a private individual. I have not the means to spend many thousand dollars to get accurate results that can be put before the public, as they would be put before the public if the experiments were instituted by the Navy Department, or the Stevens Institute, or some person of immense wealth, who could carry the matter through. That I cannot do. I have, however, made certain experiments that have cost me, I suppose, three or four thousand dollars, perhaps five thousand dollars, during the last two or three years. I advance those things as facts, and I endeavor to present a demonstration of why they are so. If the gentleman does not comprehend that, he does not comprehend anything I say.

Mr. Le Van: I would state that next week experiments will be made on the Delaware River by Mr. Roach with a similar screw placed in the bow of a vessel. I think I can get an invita-

tion for any members who wish to go on the excursion. I thought it was Mr. Root's screw; but I spoke to him about it, and he says it is not. Mr. Roach expects to prove the matter in the course of ten days.

PROFESSOR ROBINSON: I understand that facts have been presented here which are valuable,—one, in particular, of great value in this line of investigation, namely, the fact stated by Mr. Root that he had obtained greater speed of vessel than the pitch of the screw would account for,—the pitch multiplied by the velocity. It strikes me that that is possible with this screw, and, in fact, inevitable, unless we consider the resistance of the boat as very great. The theoretical velocity of the boat from this position of the screw would necessarily be greater than the pitch multiplied by the velocity. The only reason why it should not be greater than that, is that the resistance of the boat is such that it makes such an amount of slip that the actual velocity could not come up to the theoretical velocity so nearly as to equal the velocity multiplied by the pitch. The question of the speed of the boat is one of velocity and not of force; that is, considering the motion of the boat itself. The motion of the boat would be the component of velocity and not of force. We should distinguish between component of velocity and components of force here. The resolved components of velocity would be necessarily greater than the velocity multiplied by the pitch; but the resistance of the boat may be so great that it falls behind, so that the slip of the screw is necessarily so great that it amounts to the difference between the speed corresponding to the pitch and velocity and the actual speed—in other words we might have such a resistance on the boat that the boat would have actually the speed corresponding to pitch and velocity; but if we take away that resistance then the boat may go on with a higher velocity than this, and reach a velocity greater than the velocity accounted for by the pitch and faster rotation of the screw. I think the ice-boat analogy is a good one, and in considering it in connection with that, we should consider the question as one of velocity and not of force. The only difference between this in effect and the ice-boat is, it seems to me, that the fluid in the case of the ice-boat is active instead of passive. In this case the fluid is passive and not active. The propeller is here active, and in the case of the ice-boat the sail is passive. They are just the antipodes of each other in these respects; but as to velocity the question is the

same. The same diagram for the ice-boat would necessarily bring out a greater theoretical velocity than the velocity of the wind. I consider the same true here, and agree with Mr. Emery and Mr. Root. If they go down, I am willing to go down with them on this point as to the theoretical velocity. The only question as to the actual velocity exceeding the pitch and speed of screw is one of setting back on account of resistance of the boat in the water. As to the elevation of the stern of the boat, I think it is considered desirable by naval architects that a boat should stand with the prow a little higher than the stern. The boat should not appear to be running its nose into the water. If we could lift the boat so as to leave a mere streak on the surface of the water, it would of course meet with less resistance. If it is a fact that the prow of the boat is too much raised ordinarily, this may be the very thing needed to bring the boat into a desirable position.

Mr. Oberlin Smith: I do not see why experts or any one else should doubt the possibility of driving a boat faster than the speed due to the pitch of the screw, any more than that we change the amount of motion in any mechanism by applying power to inclined planes or levers at different points. After all, the power obtained is merely the product of the velocity and force applied. I am inclined to differ from Mr. Root as to the theoretical advantage of applying the force in that direction. It seems to me there is neither loss nor gain in that. I do not dispute his facts, if facts they are proved to be. In regard to the practical advantage of this, it seems to me that it is due to another cause. Pushing the water downward may be the solution of the question. If that is the case, the same advantage would be obtained by putting an ordinary horizontal screw in a lower position; but of course that is not practicable. This may have a practical advantage on that account. The line of action is toward deeper water, where there is less opportunity of its being thrown up at the surface. There would be the same advantage with an inclined screw at the bow of the vessel, such as Mr. Roach is going to experiment with. I think the advantage, if there is any, shown by Mr. Root's experiment, is not in inclining the screw, but in acting upon deeper water. I do not see why putting the force in different directions —putting a less force with a greater velocity, or a greater force with a less velocity, would make any difference in the matter, any more than with a machine it makes a difference whether you

apply one pound to one lever or two pounds to a lever of half the length. The only theoretically perfect propeller in a mobile substance like water would be an endless chain of infinite length carrying floats standing perpendicularly to their line of motion, when of course there would be no action of pushing water downwards or lifting it upwards as there is in the paddle-wheel. So long as we cannot get any theoretically perfect push on the water, this is all a matter of experiment, and this screw may, for the reasons before stated, be better than the horizontal screw. I trust that it is. It is certainly a novel idea to a good many of us—applied at this great angle, although many screws are applied at a slight angle. In regard to the lifting of the vessel at the stern, it seems to me that is a mere matter of arranging ballast. As to the advantage spoken of in practical construction, I think this screw might be a good thing, and it would probably be easier to apply the power than with the long horizontal shaft.

Mr. C. T. Porter: In order to show that we are talking without having any thing to talk about, I will state some of the conditions of Mr. Root's experiment as he related them to me yesterday. He applies his screw between the two halves of a catamaran boat at a point in the rear of the middle of the boat. The effect of his application of force is to lift the boat one-tenth of its displacement out of water. The lifting force being applied behind the centre of gravity of the boat, lifts the stern more than the bow, and the centre of gravity of the boat falls in front of its centre of support, and it slides down hill in still water. That strikes my mind as being probably the philosophy of the thing when we get the facts of the case. But the facts have been withheld. I have the pleasure of communicating them precisely as Mr. Root stated them to me yesterday, and as I expected to hear them this morning. We cannot apply a screw that way to the extreme end of the stern of a vessel, it seems to me. At any rate we have not had one so applied. We are told that an experiment has been tried under certain conditions, and the exact conditions and circumstances of the experiment ought to be spread before us.

Mr. Denton: We know that negative slips have been recorded by a number of actual experiments, extending through a long series of years. We have had a number of explanations of that, all of which allow of some action of the water as a fluid, which may be caused by the motion present in that fluid independent of that put upon it by the steam that supplies the motion to the pro-

peller. The ice-boat has been cited in illustration of this fact. There we draw on the wind in a number of planes, as Mr. Wolff has correctly explained, and we draw upon no plane of the wind for all the force present in it. We get a portion of the energy from one current and that starts the boat, another then catches it, and so on. We draw upon more and more energy as a total, but never upon the total energy of any one plane of fluid, and when we get a force which equals the frictional resistance of the boat, the boat increases its speed, but this speed is greater than the velocity that would be due to any one current of wind. We cannot have more than a constant amount of energy developed by the screw, and it cannot give us any more speed unless the energy in the steam is increased. The force according to that paper is less and the speed greater. We cannot have the speed greater without having more energy. I think Mr. Porter's position in this discussion is the right one and ought to close it. The case of the negative slip is the only fact brought forward in that paper, I think. We have a ship going faster than it would with the screw set as we ordinarily set it. That has been explained by eminent men; but yet it is not dynamically explained. We have here the same effect again, and we are asked to accept a violation of the conservation of energy theory as its explanation.

Mr. Wolff: Professor Robinson in his discussion speaks, it seems to me, of the resolution of velocities, while the question of screw propulsion is a question of the resolution of forces. The problem has been stated very clearly by Mr. Denton. We have a certain power, and that has to do a certain amount of work. We cannot possibly increase the speed, providing the power remains the same. Mr. Denton has put this forward so clearly that I do not feel called upon to do it again; but I thought I would like to indorse Mr. Denton's remarks upon this subject, and to bring out more clearly that I had those in view when I made my previous remarks.

Professor Sweet: I should like those who believe in the theory of the negative slip to take this into consideration. Supposing the power used in driving the vessel to be all consumed in keeping the water in motion, taking out of consideration the question of atmospheric resistance, and all this water in motion be concentrated into a cylinder at the rear of the vessel exactly the same in diameter as the diameter of the screw. Now if the power of the screw is lost in putting the vessel in motion, ought

it not to bring this cylinder of water exactly to rest? And if so, action and reaction being equal, where comes in the negative slip with everything favoring such a result?

MR. C. T. PORTER: I do not want Mr. Root to imagine that the somewhat sharp language I used was dictated by any feeling except this: I always want to get hold of the facts of the case, and the whole of them, and I think talking wasted until we have them all before us. Now it may be that this is a really great discovery of Mr. Root's. It may be that if we rely upon gravity to move a vessel, lifting the stern of the vessel so that gravity will propel it, that that will be something worth while doing; but that the screw exerts any force in any other direction except on the line of its axis, is quite too preposterous, in my judgment, to be credited for an instant. If the screw lifts the stern of the vessel and gravity carries it forward, that may be a very nice thing; but I submit that Mr. Root did not present anything of that kind in his paper, but withheld all the facts that would enable us to form any such theory. I wanted to express my disappointment that the facts of the case were not presented here.

MR. J. B. ROOT: I think some of the gentlemen have misapprehended the points in the paper which I read. They will see it clearly enough when they come to read the thing over. I certainly said in the paper that it was a double-hulled boat that I experimented on. In regard to the gain that can be produced, I can just say one word. The way that I account for the gain is this: We all know that a screw propeller is something that is anywhere between a revolving disk and a revolving paddle. If you increase the pitch until you bring the pitch around on a line with the axis of the screw, you have a paddle-wheel. If you decrease the pitch the other way you have got a disk. Anywhere between those two points you have got side action. That side action is a dead loss so far as creating any thrust in the line of the axis is concerned. The nearer you approach to the paddle-wheel the more side action you get. If a paddle-wheel was submerged in the water it would act as a fan merely to turn the water around; but just so soon as you make it a screw, it begins to propel the boat. If you turn the blades around still more, you get more power to push the boat ahead, and less to revolve the water; and so you can go on until you reach a point where the amount of power,—the amount of loss that is consumed in turning the water round, is counterbalanced by the friction of the blades and

the resistance of the thickness of the blades passing through the water; that is the point at which you must stop reducing your pitch. Where you reach the point where those two lines are equal, there you must stop reducing your pitch; that is the point of best result. That point has been found by experience, I think, to be at about one and a half diameters of pitch.

Now, all that I claim is this: That with a horizontal screw we have reached this point. When we give it one and a half diameters of pitch properly applied to the vessel, it is working in the best form for the horizontal screw. If we can find any way by which we can reduce the side action of the screw without increasing these losses that occur from friction and from thickness of blade, why then we get a positive gain. Now I claim, that by inclining the angle of the shaft you virtually increase the pitch of the screw. When you get the angle of shaft at 60° from the horizontal, you can get the same forward motion on a vessel with a screw of one-half the pitch. Between a screw of one and a half diameter of pitch and one of three-quarters I made a comparison, which showed a result 37 per cent. in favor of the screw of least pitch, providing you can apply that screw so as not to have that gain offset by the increase of friction. Now I say that by inclining the angle of the shaft, you do that very thing. You are enabled to use less pitch of screw, getting less loss by side action, without increasing very materially your loss from frictional surface and thickness of blade. These two losses increase as the square of the velocity, and of course increase very fast. I claim that you can reduce the pitch very materially and get a very large profit.

Professor S. W. Robinson: I do not disagree with Mr. Denton or Mr. Wolff as to energy. The same amount of energy will propel at the same speed. What I said was that this question was one of speed rather than velocity. I would like to add one word further as to the possibilities of this improvement, not yet touched upon,—it may solve the problem of the flying machine; you may yet see boats flying over the sea with nothing but the screw touching the water.

Mr. F. B. Allen: It seems to me that there will be a loss from the elevation of the stern and the depression of the bow, and as I understand it, the propeller shaft, applied as it is in an angular direction, necessitates the employment of gearing, the engine being bolted to the keelson in the usual way. That would bring an additional loss and some practical difficulties. It seems to

me that those are practical considerations worthy of our attention.

Mr. Cotter: I entirely agree with Mr. Porter in this matter. We have nothing to talk about; we are asked to accept a theory. If Mr. Root will tell us how much coal he consumed to run a certain distance in a given time, with a given displacement, with his improved screw as compared with the ordinary screw, and also, how much time he saved in running the same distance, the pressure on and velocity of his pistons being the same in both cases, then we shall have something to talk about.

Mr. J. B. Root: The gentlemen can figure up themselves how much coal it is going to take. Next year I will give them the solution of the problem, and I hope that they can then see whether they are right or wrong.

LII.

MILL FLOORS.

BY C. J. H. WOODBURY, BOSTON, MASS.

LOADS ON FLOORS.

In designing a mill floor, the first consideration must be given to the weight which it will be called upon to support; secondly, to the other forces acting upon it, the amount of flexure allowable under the fixed conditions; and then the methods of resisting those forces compatible with mechanical construction, convenience, and safety.

The following table gives the floor areas, storage capacity, weights per cubic foot and per square foot of merchandise. The measurements were always taken to the outside of case or package, and gross weights of such packages are given.

MATERIAL.	MEASUREMENTS.		WEIGHTS.		
	Floor space.	Cubic feet	Gross.	Per sq. foot.	Per cubic ft.
Wool.					
Bale East India, ...	3.0	12.	340	113	28
" Australia, ...	5.8	26.	385	66	15
" South American, . .	7.0	34.	1000	143	29
" Oregon, ...	6.9	33.	482	70	15
" California, ...	7.5	33.	550	73	17
Bag Wool, ...	5.0	30.	200	40	7
Stack of Scoured Wool, .	. . .	. . .	. . .	. . .	5
Woollen Goods.					
Case Flannels, ...	5.5	12.7	220	40	17
" Flannels, heavy, . .	7.1	15.2	330	46	22
" Dress Goods, . . .	5.5	22.0	460	84	21
" Cassimeres, . . .	10.5	28.0	550	52	20
" Underwear, . . .	7.3	21.0	350	48	16
" Blankets, ...	10.3	35.0	450	44	13
" Horse Blankets, . .	4.0	14.0	250	63	18
Cotton.					
Bale, ...	8.1	44.2	515	64	12
" Compressed, . . .	4.1	21.6	550	134	25
" Dederick Compressed,	1.25	3.13	125	100	40
" Jute, ...	2.4	9.9	300	125	30
" Jute Lashings, . .	2.6	10.5	450	172	43
" Manila, ...	3.2	10.9	280	88	26
" Hemp, ...	8.7	34.7	700	81	20
" Sisal, ...	5.3	17.0	400	75	24
Cotton Goods.					
Bale Unbleached Jeans, .	4.0	12.5	360	72	24
Piece Duck, ...	1.1	2.3	75	68	33
Bale Brown Sheetings, .	3.6	10.1	235	65	23
Case Bleached Sheetings, .	4.8	11.4	330	69	30
Case Quilts, ...	7.2	19.0	295	41	16
Bale Print Cloth, . . .	4.0	9.3	175	44	19
Case Prints, ...	4.5	13.4	420	93	31
Bale Tickings, ...	3.3	8.8	325	99	37
Skeins Cotton Yarn, . .	. . .	. . .	. . .	. . .	11
Burlaps, ...	. . .	. . .	130	. . .	30
Jute Bagging, ...	1.4	5.3	100	70	24
Rags in Bales.					
White Linen, ...	8.5	39.5	910	107	23
White Cotton, ...	9.2	40.0	517	78	18
Brown Cotton, ...	7.6	30.0	442	59	15
Paper Shavings, . . .	7.5	34.0	507	68	15
Sacking, ...	16.0	65.0	450	28	7
Woollen, ...	7.5	30.0	600	80	20
Jute Butts, ...	2.8	11.1	400	143	36
Paper.					
Calendered Book, . . .	. . .	. . .	. . .	. . .	50
Super Calendered Book, .	. . .	. . .	. . .	. . .	69
Newspaper, ...	. . .	. . .	. . .	. . .	38
Straw Board, ...	. . .	. . .	. . .	. . .	33
Leather Board, ...	. . .	. . .	. . .	. . .	59
Writing, ...	. . .	. . .	. . .	. . .	64
Wrapping, ...	. . .	. . .	. . .	. . .	97
Manila, ...	. . .	. . .	. . .	. . .	37

Material.	Measurements.		Weights.		
	Floor space.	Cubic feet	Gross	Per sq. foot.	Per cub c ft.
Grain.					
Wheat in Bags, ...	4.2	4.2	165	39	39
" in Bulk, ...	. . .	. . .	. . .	. . .	44
" " "	. . .	. . .	. . .	. . .	39
" " " mean, . .	. . .	. . .	. . .	. . .	41
Barrels Flour on side, . .	4.1	5.4	218	53	40
" " on end, . .	3.1	7.1	218	70	31
Corn in Bags, ...	3.6	3.6	112	31	31
Cornmeal in Barrels, . .	3.7	5.9	218	59	37
Oats in Bags, ...	3.3	3.6	96	29	27
Bale of Hay, ...	5.0	20.0	284	14	57
Hay, Dederick Compressed,	1.75	5.25	125	72	24
Straw, " " .	1.75	5.25	100	57	19
Tow, " " .	1.75	5.25	159	86	29
Excelsior, " " .	1.75	5.25	100	57	19
Dye Stuffs, etc.					
Hogshead Bleaching Powder,	11.8	39.2	1200	102	31
" Soda Ash, . . .	10.8	29.2	1800	167	62
Box Indigo, ...	3.0	9.0	385	128	43
" Cutch, ...	4.0	3.3	150	38	45
" Sumac, ...	1.6	4.1	160	100	39
Caustic Soda in iron drum,	4.3	6.8	600	140	88
Barrel Starch, ...	3.0	10.5	250	83	23
" Pearl Alum, . . .	3.0	10.5	350	117	33
Box Extract Logwood, . .	1.06	.8	55	52	70
Barrel Lime, ...	3.6	4.5	225	63	50
" Cement, American,	3.8	5.5	325	86	59
" " English, .	3.8	5.5	400	105	73
" Plaster, ...	3.7	6.1	325	88	53
" Rosin, ...	3.0	9.0	430	143	48
" Lard Oil, . . .	4.3	12.3	422	98	34
Rope, ...	. . .	. . .	. . .	. . .	42
Miscellaneous.					
Box Tin, ...	2.7	0.5	139	99	278
" Glass, ...	. . .	. . .	. . .	. . .	60
Crate Crockery, ...	9.9	39.6	1600	162	40
Cask Crockery, ...	13.4	42.5	600	52	14
Bale Leather, ...	7.3	12.2	190	26	16
" Goatskins, ...	11.2	16.7	300	27	18
" Raw Hides, ...	6.0	30.0	400	67	13
" " compressed,	6.0	30.0	700	117	23
" Sole Leather, . . .	12.6	8.9	200	22	16
Pile Sole Leather, . . .	. . .	. . .	. . .	. . .	17
Barrel Granulated Sugar,	3.0	7.5	317	106	42
" Brown Sugar, . .	3.0	7.5	340	113	45
Cheese, ...	. . .	. . .	. . .	. . .	30

As a basis for general application of principles, let us take the case of a cotton-mill of 32,000 spindles, spinning number 24 yarn; such a mill being five stories in height and floor measurement being 72 by 342.

Not the best proportion of height, all things being considered, but it is one which is frequently built.

The following table of weights of machinery in such a mill was prepared by Mr. Edward Atkinson.

COMPUTATION FOR A COTTON MILL OF 32,000 SPINDLES.

No. 24 Yarn.

	Pounds, about.	Total.	Stock.	Shafting.	Operatives.	Total Weight.	Floor Space.	Average Weights.
6 Lappers......	12,000	72,000	280	13,000	300	85,580	In sep. Build'g.	
240 36 Inch Cards......	1,600	384,000	4,800	11,920	275	400,995		
2 Doublers...........	980	1,960	40	1,100	......	3,100	17,856	23.30
12 R. W. Heads.......	800	9,600	......	1,100	180	10,880		
21 Drawing...........	2,000	42,000	850	1,250	180	44,280	5,184	8.52
36 Roving Frames....	9,000	324,000	2,000	14,500	4,300	344,800	12,528	27.52
90 Spinning Frames, 176 Spindles each,	4,000	360,000	15,340	125,000	1,820	502,160	15,984	31.42
24 Mules, 672 sps. each	10,000	240,000	16,000	24,000	2,800	282,800	24,192	11.69
8 Spoolers...........	2,600	20,800	800	5,000	825	27,515	5,184	6.15
6 Warpers..	450	2,700	700	410	500	4,370		
2 Slashers...........	......	4,000	5,600	2,200	480	12,280	5,760	26.66
8 Drawing in Frames	60	480	1,600	......	1,000	3,080		
800 Looms.........	800	640,000	128,000	140,000	16,000	924,000	36,864	25.07
		2,029,540	175,880	326,480	28,360	2,560,260	123,552	20.72
	Add for contingencies 20 per cent.					512,052		
						3,072,302		24.86

The greatest concentration of weight is immediately under the slashers, and as the floor reserved around the slashers is used to pile loaded yarn beams upon, the weight at this point is relatively much more than the figures show. But the slashers do not contain a large assemblage of rapidly revolving parts, and are merely a dead weight.

The openers and pickers are omitted, as such machinery is placed in a separate building, usually constructed with beams of shorter span.

VIBRATION AND OSCILLATION OF MILLS.

If the machinery were to lie idle, the building would become a store-house containing machinery of such a weight, and the statement of data would be complete to furnish the elements for computation; but the instant that machinery is put in operation, other conditions arise, resulting from the impact of every unbalanced force in the whole train of mechanism.

In the practical construction of machinery the revolving parts

are never absolutely balanced, and but little attempt is made to balance the reciprocating parts, and when one considers the immense weight of the unbalanced masses of mill machinery and their velocity, it seems as if this aggregation of unbalanced forces would demolish instead of merely jar a mill; and such a destructive result would undoubtedly happen if the impulses of all these irregularities acted in unison.

Before citing any examples of this trembling of mills,—giving some of the numerous instances within my personal observation, or attempting any analysis of the subject,—I wish to make a distinction of terms; using, to express two different classes of motions, words which are defined and used as synonymous with each other.

Then, with the difference between vibration and oscillation understood, we will consider some examples, their consequences, and the nature of the remedy. The motion of a shaking mass is either oscillation or vibration; the first being caused by external forces, and the second the consequence of forces, one of which is the cohesive force within the body, binding its molecules together, and dependent for its character upon the elasticity of the substance. To illustrate: If a suspended wire is struck, the forces of the blow and of gravity cause it to oscillate; if the same wire is strained between two points, the forces of the blow and of elasticity cause it to vibrate.

The rapidity of this vibration is subject to well-known laws dependent upon the magnitude, elasticity, and tension of the body, and every mass will vibrate at a certain velocity, whenever that vibration is produced by adequate causes and conditions. Not only every pianoforte wire and organ pipe, but every bridge and factory has its keynote. As the vibrations of the communicating air will force the piano or organ to answer in harmonious response to the tone of the voice, so the structure will reply to the proper vibratory force. The sound will be inaudible, because the perception of the tone is beyond the limits of the human ear.

In one of the print works at North Adams a new building, not yet completed, was vibrating, evidently in synchronism with a small puffing engine, with a 4 by 6 inch cylinder, which was running the folders in the steaming-room, about 60 feet away, and in no manner connected with the new building.

The shaking of a mill is chiefly limited to the vibration of the elastic floors and beams, while the swaying of the whole building

is caused by oscillation. Both of these motions may appear in the same building, but oscillation is due to weak construction, while vibration is produced by impulses synchronous with the keynote of the building, and bears little relation to that stability of construction, which is equal to supporting the loads placed upon it.

In a certain district of New England, many factories were built during the war, when high prices, haste for completion, and perhaps limited capital, resulted in poorly constructed buildings. Shabbiness prevails everywhere,—the walls are too light, the beams too small, and the floor planks too thin, in comparison with the mills built by our best engineers as standards. In several instances the weight of the machinery has deflected the floor beams, to the dangerous limit of 3 inches, and in other cases additional columns have been used to keep the floors level. The oscillation of these mills is excessive, but I do not recall an instance of extreme vibration in any of them.

The remedies for oscillating mills consist in reconstructing the weaker portions, or furnishing the missing elements of strength by some additions supplying the lack of stability.

At a mill in Eastern Massachusetts, several trusses have been placed across the mill, above each other, in the upper three stories. In a mill in New Jersey, the same difficulty was removed by a series of horizontal Pratt trusses, under the floors, the floor beams forming the two chords, the struts reaching from beam to beam, and the whole completed by the use of proper tie rods.

The vibration of a mill floor resembles that of a metal plate, and the analogy is clearly seen by comparing the characteristics of a metal plate, vibrating under conditions which are simple and easily controlled, with the results of such vibration in mills.

If a metal plate is supported at the centre, and a violin bow be drawn across the edge, the plate will not vibrate as a whole, but in divisions, which are readily distinguished if sand has been dusted over the plate; the vibrating portions will jar the sand to the nodes or dividing lines which are at rest. If the plate is a square one and the bow drawn at the corner, while free vibration is damped by resting the finger on one side midway between the two corners, the sand will gather in two lines, dividing the plate into four equal squares; if the bow is drawn at the middle of one side and the finger placed at one of the corners, the ridges of sand will divide the plate into two diagonals extending from corner to

corner; and by varying the positions of the damping points and the bow the nodal divisions resolve themselves into complex and curious curves and lines. A vibrating mill floor is under similar conditions, except that the contact of numerous columns and surrounding walls, as far as they restrain free vibration, complicate the details of these vibrating divisions beyond the power of analysis, except to show that the floor is divided into numerous and independent divisions.

Although these areas of vibration are modified by the columns and walls, yet they are not coincident with any divisions of the floor, because excessive vibration has been observed near such places, while the portions away from such support were at rest, but the reverse is generally the case, and most perfect repose is usually near the points of greatest stability—where one would naturally expect to find it.

The roofs of mills are more generally in vibration than any other portion of the building. The motions of gas pipes or of the hanging lamps show the character and extent of such vibrations, although on account of their rigidity the gas pipes do not exhibit the divisions as sensitively as hanging lamps.

Miscellaneous collections of patterns or small castings, piled against each other over a floor, indicate by their creaking the points of greatest vibration.

Piles of paper shavings in paper mills show, by their trembling, the character of the vibration of the building.

Sprinkler pipes are generally in a tremor, because any disturbance is transmitted along the whole line of pipe. Sometimes sprinkler pipes are so sensitive to vibration that they will ring in harmony with the tones of the voice in ordinary conversation.

When the attic floor is suspended from the roof by rods it will often be found that some of these rods are vibrating rapidly while others are at rest.

In the attic of a mill at Holyoke, where the floor was hung to the roof, there were a number of piles of cardboard, from three to six feet in height. Some of these piles were shaking and others were still, according to the action of that portion of the floor upon which they rested. At one side of a tie rod from the roof the pile was swinging; on the other side it was stationary.

On pushing against the piles, those which were at rest did not appear to be any more stable than the others. When large iron tanks of water in the attics rest directly upon the floor, the

ripples on their surface show the character of the motions of the floor.

One of the most delicate indications was in the unoccupied attic of a mill at Exeter, N. H., where the agent had judiciously placed upon the floor numerous fire pails, filled with water. The surfaces moved in unison with the vibration of the floor, generally swaying, some traversed by successive lines of waves, whose direction varied in different pails. Others, evidently over nodal lines, were at rest, while a few which were situated upon centres of vibration were covered with ripples, such as are produced when a hanging pail of water is struck underneath. Of two pails, situated within a foot of each other, one indicated by its ripples that its position was over a centre of greatest vibration, while the other showed by its wave-like motion that its location was near such a point of greatest vibration, but not over it. The vibrations of floors are in waves moving up and down, without any progression, and the character of the motions of the water in these pails was concurrent with such action.

In the rag building of one of the largest paper mills in this country are large bins, the sides, which are of thin boards, extending from floor to ceiling. In the bin-room are numerous fire-pails, hung in groups upon hooks against the sides of these bins. As the hooks are rather short the edges of the pails rest against the bins. In each group of six pails, hung side by side, there is generally a difference in the wave motion of the water in each one. These bins are in vibration and, presumably, the whole building also. There is no machinery in this building, but in an ell are cutters, dusters, and a large exhaust blower, whose suction pipe extends into the rag-sorting room, above the room containing the pins, for purposes of ventilation. It is probable that the vibration of this air pipe shakes the whole building.

It is well known that some notes on a church organ will cause only certain portions of that church to vibrate.

At certain heights of water, the vibration of the sheet of water flowing over a mill-dam is synchronous with the keynote of certain buildings in the vicinity. If the rate of vibration of the dam can be altered the buildings come to a rest.

At Centredale, R. I., the water flowing over the dam caused at times a great vibration in the mill. This has been stopped by fastening vertical pieces of plank at intervals of ten feet against the front of the dam and projecting upwards, so as to break the

long sheet of water into numerous short falls, whose keynote was different from that of the whole sheet of water which they displaced.

At the Amesbury Mills of the Hamilton Woollen Company, mill No. 8 is sometimes thrown into vibrations by the water flowing over the dam, and when this happens the watchman on duty alters the note of the dam by opening the waste gate and the mill soon comes to rest. Mill No. 5 of the same company, but at a privilege lower down the stream, is often thrown into vibration by the falling water at that point, and the story is told that a watchman once fled from that mill in the night, thinking that it was about to fall.

Of the eleven mills owned by this corporation, I have been told that none of the others vibrate from the above cause. When in operation, mill No. 6 was often thrown into vibration by the motion of the machinery.

The superintendent of a New Hampshire mill when in the factory on Sunday was greatly surprised at the shaking of the building. He observed that the water was flowing in broken sheets over the dam and presumed that the pulsations of air were synchronous with the keynote of the mill. Continuing his observations, he learned that with a greater or a smaller flow of water over the dam the mill was at rest, and also with the scuttle in the roof open, so that the puffs of outside air were in free communication with the interior of the mill, the vibration was much greater than when the scuttle was closed.

One of the mills in West Warren, Mass., was shaking violently during an evening after the machinery had stopped. The water flowing over the dam was undoubtedly the cause. Some of the windows were shaking and others were still, as if there were points of greatest vibration either of the walls or the air within the building corresponding to the nodes in an organ-pipe. At Mittineague, Mass., the Southworth Paper Company is vibrated when the water is about a foot upon the dam. The cotton mill of the Agawam Canal Company, near by, is vibrated at a different height of water. When the writer was there, he noticed that the windows of the office building jarred at intervals of fourteen seconds. Is it possible that such a slow succession of impulses is due to beats?

In making some experiments with a spinning-frame in the winter of 1879 I met with some very curious vibrations.

A board five feet long was fastened at each end to the side of

the frame, parallel to the rails and about five inches from them. At times this board would vibrate and then come to a rest every thirty seconds. A number of persons saw it and many hypotheses were offered, but none of them satisfactorily accounted for the two rates of impulses which caused this result.

Spinning frames indicate the vibration more sensitively than any other class of textile machinery. In the same spinning-room, where the frames are of the same manufacture and supposed to be operating under identical conditions, some of them will shake violently, while others beside them run as steadily as could be desired.

When vibrations are due to machinery, they are stopped by changing the speed. Two years ago, I was in a mill in Haydenville, Mass., and noted the vibration, which exceeded anything that I ever saw, judging from the motion of the hanging lamps. Recently I visited the mill again, and was surprised at its steadiness. In the interval, the property had changed owners, and the new proprietor had increased the speed about ten per cent. I was informed that there had been no other alteration in the main mill or its equipment, which would assist in bringing about any change of vibration.

At the Merrimac Mills, Lowell, and many other places, this method of stopping vibration by changing speed has been successful.

The synchronous vibration of a mill can be observed in many instances where the machinery soon after starting shakes the mill, before reaching full speed, and the building becomes steady again after the velocity of the machinery exceeds a certain limit.

In all cases vibrations are noticed when some adequate cause is sounding the keynote. If the velocity of these pulsations is altered the vibrations due to that identical cause will cease.

There is a practical difficulty in the way of reducing vibration by changing the speed of a portion of the machinery in a mill. There would be an objection to diminishing the speed on account of reduced production; and if the speed of a portion of the machinery could be successfully increased, the promise of enlarged production would cause an attempt to increase the speed of all the machinery. The most practicable method in many instances is to change the keynote of the building by some alterations of the conditions of vibration.

Were not a lot of columns, placed almost at random, out of the

question in a mill, we could make the statement that vibration could be diminished and nearly stopped by placing columns under the principal centres of vibration and so divide the floor into still smaller areas of vibration.

A similar result has been obtained by placing braces near the tops of the columns, extending out to the ceiling.

The only sure method of prevention applicable to the future seems to be in the construction of one-story mills, with the shafting resting upon piers in the basement. The result in the additional speed, without any disturbance of stability, and of the diminution of the repairs under the same excessive conditions in mills constructed upon the one-story plan will hasten their introduction. In such a mill, if vibration should take place, it could be stopped by additional columns in the low basement without interfering with the machinery.

The consequences of mill vibration in the power absorbed by its exertion in the continual straining of machinery and mill must inevitably tend to damaging results to plant and product.

The power required to vibrate a mill, bending the floors, walls, and machinery to and fro, is generally underestimated. One of the leading manufacturers of spinning machinery stated to the writer, as the result of careful experiment, that it required twenty per cent. more power to operate spinning frames in a mill which vibrated than in one which was steady.

In ordinary mills, the stability of the building limits the speed of most textile machinery, but when vibration is reduced to a minimum, as in the case of a one-story mill, the limit of speed is a question of machine construction. In an alpaca weaving mill, one story high, the looms are running twelve and a half per cent. faster than would be possible in a high mill.

One of our New England corporations built a one-story mill, and put into it one thousand gingham looms, which were removed from a three-story mill in the same yard. The result of three years' experience has shown that it is perfectly feasible to run these looms twelve per cent. faster than was possible in the old mill, and the speed of certain varieties of looms has been increased twenty-one per cent. The greater steadiness and uniformity of motion has decreased the number of broken threads, even at this additional speed. Although these looms have been growing older, and are subjected to the additional wear due to increased speed, the steadiness of motion has reduced the amount of repairs,

which is estimated at ten per cent. As it was necessary to readjust all the looms to the increased speed, which was a part of the repair account, the relative cost which renewals bore to readjustments could be only a matter of estimation.

A one-story mill lighted from above is much better lighted than is possible in any mill lighted by windows. In the case of this one-story mill the annual cost of lighting these looms by gas through the meter from the public gasworks was less than lighting the same machinery in the old mill, by an annual saving equal to six per cent. on the cost of the one-story mill. That is, if the new mill had been built with money borrowed at six per cent., the saving in cost of lighting would equal the interest on the loan.

However great may be the advantages from low construction, it is feasible only on cheap and level land, and, therefore, can rarely be applied to the extension of mills built near streams or in cities.

The best support for heavy machinery, as cotton-pickers, wood and iron planers, and all machinery where a substantial foundation is essential, is made of timber six or eight inches square, laid upon a solid foundation at distances from each other convenient to bolt the machinery directly into the timbers. The space between the timbers to be filled with small stones well rammed and covered with asphalt concrete, finished smooth to the level of the tops of these timbers. The asphalt preserves the wood from decay, is a poor conductor of heat, is agreeable to stand upon, is waterproof, and should be painted to protect it from being softened by oil from the machinery.

Cement should never be used in the place of asphaltum concrete, as the timber will decay very quickly. Some asphalt floors are composed of nine parts Neuchâtel (Val de Travers) asphalt, one part Trinidad asphalt, and a small amount of clean sand; others have used only the Trinidad asphalt. The cost of such a floor an inch and a half thick, on earth with rubble foundation, is eighteen to twenty two cents per square foot.

STRENGTH OF FLOORS.

Wood is both the best and the most practicable material for the construction of floors for industrial purposes. Floors made of brick arches sprung between I beams are so heavy and expensive as to be rarely feasible. In some mills so constructed, with iron beams resting upon iron columns, it has been necessary to raise

the beams and place wood under their supports at the walls and over the columns. When the Boston and Lowell Railway was built, the rails were laid upon stone ties, which were so rigid that the rolling stock battered itself at every bearing and joint; and the cause of this difficulty ceased when the stone ties were replaced by those of wood.

Southern pine is a very desirable wood for beams, as it possesses the essential qualities of strength, elasticity, straight grain, and less tendency to curl and warp than any other wood in our market, while its abundance, in certain sections, and massive growth renders it the most practicable kind of lumber to obtain in the large dimensions necessary for mill work.

For under plank of floors, spruce is a good lumber, being abundant, cheap, light, and quite strong; but it should be thoroughly seasoned before used, because in drying it warps and shrinks considerably.

For the top floors, Southern pine, maple, and birch are the best woods; their respective utility varying with the kind of wear which comes upon the floor.

The strength of wood is a very indefinite quantity, varying in the same specimen with the time and application of the stress; in different pieces of the same kinds of wood there is also a certain diversity due to the differences of structure caused by varying conditions of growth and quality.

The proper unit of strength which *should be* used in designing structures which merely support weight is not the breaking strength of the material, but the elastic limit which will sustain that load without injurious flexure. The detrimental effect of continual strain in causing a fatigue of the fibres of wood is conclusively shown by the experiments of Professor R. H. Thurston * and those of Sir William Fairbairn, where beams loaded with various portions of their instantaneous breaking weight, eventually broke. A load less than the elastic limit would be sustained indefinitely. In the lack of information giving the elastic limit of wood, it is necessary to use with the breaking strength, or modulus of rupture, a factor of safety large enough to allow for the excess of this unit of strength beyond the elastic limit, in addition to the ordinary provision for imperfection and deterioration of material. For store-house floors, well built, of

* Journal Franklin Institute, September, 1881; also, Application of Cast and Wrought Iron to Building Purposes. William Fairbairn, London, 1854.

sound material, I consider six [6] the minimum factor of safety for dead loads, and for live loads in all cases a double factor should be used.

The formulæ which apply to the construction of floors are deduced in the following manner: Let

h represent the depth of beam in inches.
b " " breadth of beam in inches.
d " " deflection of beam in inches.
l " " span of beam in feet.
s " " width of load of beam in feet.
x " " distance of any transverse section from end of beam.
w " " load per square foot of floor, including its own weight in pounds.
w' " " load upon square foot of floor, not including weight of floor, in pounds.
u " " weight of floor in pounds per square foot.
W " " concentrated load in pounds.
R " " modulus of rupture in pounds.
E " " modulus of elasticity in pounds.
f " " factor of safety in units.

In a beam supported at both ends, and sustaining a uniformly distributed load, of w pounds to the square foot, width s and length l, extending the whole length of the beam, the maximum bending moment is at the centre of the span:

$$M_b = \frac{3wsl^2}{2} \text{ inch pounds.}$$

This bending moment is resisted by the strength of the beam expressed in terms of the moment of resistance.

$$M_r = \frac{Rbh^2}{6} \text{ inch pounds.*}$$

* The modulus of rupture is the utmost strain which the fibres of the substance will bear, either tension at the bottom or compression at the top, whichever is the least, in a beam subjected to transverse strain. This coefficient is obtained by experiments upon the weight required at the centre of a beam supported at both ends to break the beam. In these measurements upon beams of long span the weight of the beam should be taken into account.

$$\frac{Rbh^2}{6} = 3Wl + \frac{3wl^2}{2}$$

$$R = \frac{18Wl + 9wl^2}{bh^2}$$

If the beam supports the load,

$$\frac{3wsl^2}{2} < \frac{Rbh^2}{6}$$

$$w < \frac{Rbh^2}{9sl^2}$$

Introducing the factor of safety, these formulæ give the following for beams:

(1) Weight in pounds per square foot of floor, $w = \dfrac{Rbh^2}{9fsl^2}$

(2) Span in feet, $l = \sqrt{\dfrac{Rbh^2}{9wfs}}$

(3) Height of beam in inches, $h = \sqrt{\dfrac{9wfsl^2}{Rb}}$

In the case of floors bearing a uniformly distributed fixed load, this formula for the floor plank reduces to

(4) Weight in pounds per square foot of floor, $w = \dfrac{4Rh^2}{3fl^2}$

(5) Span between centres in feet, $l = \sqrt{\dfrac{4Rh^2}{3wf}}$

(6) Thickness of floor in inches, $h = \sqrt{\dfrac{3wfl^2}{4R}}$

These formulæ would only apply in those mill floors whose dimensions were based upon the safe load, or in structures where the total load was always stationary, or in storehouses where the load is in bulk, as in grain storehouses, where the moving of the contents is so gradual as to be unaccompanied by any sudden

---

If the beam is one foot long and one inch square,

$$R = 18W + 9w.$$

If the weight of the beam is neglected,

$$R = 18W.$$

Some works give this value of W as a basis of breaking strength.

shock. For storehouse floors loaded with goods in bale, case, or package, this formula will not apply, because it does not, as is generally assumed, meet the conditions of the case, and a slightly different treatment of the problem is necessary. The maximum load on the beams of a storehouse may, without error on the side of danger, be considered an evenly distributed, fixed load, because the additional strains due to moving the contents are not only a very small proportion of the whole weight sustained by each beam, but, from the nature of things, whenever any portion of the storage is moved the floor sustained by that beam cannot be wholly covered, and therefore carries less than its maximum load. The planks in a storehouse floor are acted upon by two forces: first, their own weight, which is an evenly distributed, fixed load, and, secondly, the weight of the contents, which is a concentrated live load, whenever the cases or bales are moved about in the customary manner on two-wheeled trucks.

The maximum bending moment of a concentrated load being twice that of an equal uniformly distributed load, and the straining effect of a live load being twice that of a dead load, whenever the contents of a storehouse are moved, the floor planks are subjected to strains four times as great as when the same load is at rest, therefore this condition of maximum strains will only be considered.

Bending moment due to weight of floor, $M_b = \dfrac{3wl^2}{2}$ inch pounds.

Bending moment due to load on floor, $M^1_b = 3\,Wl$ inch pounds.

Introducing a factor of safety f for the fixed load, and the double factor $2f$ for the live load, and writing W in terms of the area of the floor, these formulæ reduce to $M_b = \dfrac{3wfl^2}{2}$ inch pounds.

$$M^1_b = 6w^1fl^2 \; *$$

The moment of resistance of the floor for a section twelve inches in breadth.

$$M_r = \frac{Rbh^2}{6} = 2Rh^2.$$

* w^1 represents only the superincumbent load upon the floor, and does not include the proportionate part of the weight of the floor, like w in the other formulæ, where the weight of the floor is not treated separately from the load upon it.

The sum of the safe bending moments equals the moment of resistance.

$$6w^1fl^2 + \frac{3ufl^2}{2} = 2\,Rh^2.$$

(7) Load in pounds per square foot of floor, $w^1 = \frac{4Rh^2 - 3ufl^2}{12fl^2}$

(8) Span between centres of beams in feet, $l = \sqrt{\frac{4Rh^2}{3f(4w^1 + u)}}$

(9) Thickness of floor in inches, $h = \sqrt{\frac{3fl^2(4w^1 + u)}{4R}}$

The following table gives the dimensions of a storehouse floor built of spruce plank laid upon southern pine beams eight feet between centres. The breadth of the beams is half their height.

This proportion limits the intensity of pressure against the beams at the columns and points of support to within the crushing resistance of the wood.

The modulus of rupture of spruce is taken as 10,080 pounds, and of southern pine, 12,960 pounds for sound lumber.

Substituting these values in equation (1) for the beams,

$$w = \frac{Rbh^2}{9fsl^2}.$$

(10) Weight in pounds per square foot of floor, $w = \frac{15h}{l}$

(11) Span in feet, $l = \sqrt{\frac{15h^3}{w}}$

(12) Height of beam in inches, $h = \sqrt[3]{\frac{wl^2}{15}}$

For the floor plank, substituting these values in equation (7)

$$w^1 = \frac{4Rh^2 - 3ufl^2}{12fl^2}$$

(13) Load in pounds per square foot of floor, $w^1 = \frac{35h^2 - u}{4}$

(14) Span between centres of beams in feet, $l = \sqrt{\frac{2240h^2}{4w^1 + u}}$

(15) Thickness of floor in inches, $h = \sqrt{\frac{4w^1 + u}{35}}$

Table of dimensions for a storehouse floor for goods in bales, packages, or cases.

Built of spruce plank laid upon southern pine beams eight feet between centres. Weight of spruce per cubic foot, 30 pounds; of southern pine, 48 pounds. Timber completely sheltered from the weather.

Weight per Square Foot of Floor.				Dimensions of Beams.			Thickness of Floor Plank. Inches.
Storage.	Weight of Beam. Pounds.	Weight of Floor Plank.	Total.	Height. Inches.	Breadth. Inches.	Span. Feet.	
w^1		u	(10) $w = \frac{15h^3}{l^2}$	(12) $h = \sqrt[3]{\frac{wl^2}{15}}$	$b = \frac{h}{2}$	(11) $l = \sqrt{\frac{15h^3}{w}}$	(15) $h = \sqrt{\frac{4w^1 + u}{35}}$
50	3.00 4.08 5.33	6.06	59.06 60.14 61.39	12 14 16	6 7 8	20.96 26.16 31.64	2.43
75	3.00 4.08 5.33	7.43	85.43 86.51 87.76	12 14 16	6 7 8	17.42 21.81 26.46	2.96
100	3.00 4.08 5.33	8.54	111.54 112.62 113.87	12 14 16	6 7 8	15.24 19.12 23.23	3.41
150	3.00 4.08 5.33	10.37	163.37 164.45 165.70	12 14 16	6 7 8	12.60 15.78 19.26	4.17
200	3.00 4.08 5.33	12.03	215.03 216.11 217.36	12 14 16	6 7 8	11.11 13.80 16.81	4.82
250	3.00 4.08 5.33	13.53	266.53 267.61 268.86	12 14 16	6 7 8	9.86 12.40 15.12	5.38
300	3.00 4.08 5.33	14.72	317.72 318.80 320.05	12 14 16	6 7 8	9.03 11.36 13.86	5.89

Several sizes of beams are given, so that a selection of those which will apply most conveniently to any specific case may be made. The height of stories should be built with the distance in the clear so low that it will be impossible to place a dangerous load of goods upon the floor.

As many storehouses are still constructed according to the objectionable custom of laying the floor upon joists laid trans-

versely upon the beams, the formulæ for the joists are in this form. Let s in this case represent the distance in feet between centres of joists.

$$\frac{Rbh^2}{6} = 6w^1 sfl^2 + \frac{3usfl^2}{2}$$

(16) Load in pounds per square foot of floor on each joist,

$$w^1 = \frac{Rbh^2 - 9usfl^2}{36sfl^2}$$

(17) Span of joist in feet, $l = \sqrt{\frac{Rbh^2}{9sf'(4w^1 + u)}}$

(18) Height of joist in inches, $h = \sqrt{\frac{9sfl^2(4w^1 + u)}{Rb}}$

STIFFNESS OF FLOORS.

The limit of stability required for floors sustaining machinery in operation is not merely the strength adequate to support the load with safety, but a certain degree of stiffness is necessary, so that the deflection of the floor will not be detrimental to the machinery or its product. The limits of deflection here given are not the greatest that ever take place, nor is any slight excess or deflection beyond these limits followed by sudden impairment or destruction of building or machinery; but these coefficients are based upon a careful observation of mills built by leading engineers as standards, and are presumed to represent the best practice of mill engineering construction of to-day.

The driving shafting being hung from the beams which extend transversely across the mill, its alignment is not materially affected by their deflection, because the load on the floor is permanent and bends the beams quite uniformly, depressing the whole line of shafting, and the error of the alignment due to this cause and the shrinking of the timber can be annulled by adjusting the boxes in the hangers to their new position.

The first limit for the deflection of such beams is the elastic limit of the wood.

Experience has shown that such floor beams deflected one-four-hundredth of their span, or three-hundredths of an inch per foot, are within the elastic limit, and there is no appreciable increase of deflection.

In the matter of supporting machinery, the most important ele-

ment is to determine the maximum limit of the depression of the floor. Machinery is frequently subjected to excessive flexure from this cause, and the result of such distortion upon the wear of machinery, excessive consumption of power, and detriment to production are well-established facts.

Machinery should be carefully levelled after being placed in position; but this adjustment does not warrant an unusual deflection of the floor, because the impact of vibration is a live load whose elements are somewhat uncertain, but they should be considered equal to the weight of the mechanism; and being independent of gravity, their components are as frequently upward as downward, and the floor plank should be equally rigid against forces in either direction.

Although high, narrow beams, within the limits of crushing at the supports or crippling sideways, are the most economical distribution of material, the need of lateral rigidity to withstand the horizontal components of vibration confines the ratio of the breadth to the depth of a beam in a mill to a greater proportion than is advisable in a storehouse.

Tredgold, in 1822, gave one-twelve-hundredth of the span as the limit of deflection for cast-iron shafting, and other writers since then have applied this factor as the limit of all shafting. I have never seen any statement of the allowable distortion of a whole machine. The limit of such deflection varies with the class of mechanism. In cotton machinery, a deflection of the floor plank amounting to one-twelve-hundredth of the span agrees with the precedents of good construction.

This deflection of the floor represents a still smaller distortion of the machines upon it, and in the absence of any precise data respecting this limit, the desired result can be reached by taking a floor-deflection which is approved by experience as representing a certain multiple of the flexure of the machinery upon the floor.

The floor should be rigid within this limit in both directions. As the most convenient distance between centres of beams in mills is eight feet, the limit of deflection of the plank is about one-thirteenth of an inch; and the deflection of the beams, from each chord of eight feet along their length, should be the same.

This subject will be considered in further detail, with numerical examples, after the equations of the elastic curve have been given.

Within such limits that the stress is proportional to the strain, the deflection * of such a beam is found by the calculus to be

$$(19)\ d = \frac{864wsl^4}{Ebh^3}\left(\frac{x^4}{l^4} - \frac{2x^3}{l^3} + \frac{x}{l}\right)$$

$$* \frac{d^2y}{dx^2} = \frac{M}{EI}$$

In a uniformly loaded beam l inches long, $M = \frac{ws}{2}(lx - x^2)$.

$$d^2y = \frac{ws}{2EI}(ix - x^2)$$

$$\frac{dy}{dx} = \frac{ws}{2EI}\int(lx - x^2)\,dx$$

$$\frac{dy}{dx} = \frac{ws}{2EI}\left\{\frac{lx^2}{2} - \frac{x^3}{3}\right\} + C$$

Beam is horizontal at centre where $x = \frac{l}{2}$ and tangent to angle of depression $\frac{dy}{dx} = o$.

$$C = -\frac{ws}{2EI}\left\{\frac{l^3}{8} - \frac{l^3}{24}\right\} = -\frac{wsl^3}{24EI}$$

substituting

$$\frac{dy}{dx} = \frac{ws}{2EI}\left(\frac{lx^2}{2} - \frac{x^3}{3}\right) - \frac{wsl^3}{24EI}$$

$$\frac{dy}{dx} = \frac{wslx^2}{4EI} - \frac{wsx^3}{6EI} - \frac{wsl^3}{24EI}$$

$$\frac{dy}{dx} = \frac{ws}{24EI}(4x^3 - 6lx^2 + l^3)$$

$$y = \frac{ws}{24EI}\int(4x^3 - 6lx^2 + l^3)\,dx$$

$$y = \frac{ws}{24EI}\left\{\frac{4x^4}{4} - 2lx^3 + l^3x\right\} + C$$

$$y = o \text{ for } x = o, \text{ therefore } C = o,$$

$$y = \frac{wsl^4}{24EI}\left(\frac{x^4}{l^4} - \frac{2x^3}{l^3} + \frac{x}{l}\right)$$

In a rectangular beam $I = \frac{bh^3}{12}$ and substituting the value of l in feet, instead of inches, we obtain the equation,

$$y = \frac{864wsl^4}{Ebh^3}\left(\frac{x^4}{l^4} - \frac{2x^3}{l^3} + \frac{x}{l}\right)$$

From this equation of the flexure of the beam supported at both ends, the maximum deflection of the beam at centre of span is obtained by substituting $x = \frac{l}{2}$.*

(20) The deflection in inches, $d = \frac{270wsl^4}{Ebh^3}$.

(21) Weight per square foot of floor in pounds, $w = \frac{Ebh^3d}{270sl^4}$.

(22) Height of beam in inches, $h = \sqrt[3]{\frac{270wsl^4}{Ebd}}$.

If the floor plank are only long enough to reach from centre to centre of beam, $s = 1$ foot, and $b = 12$ inches, and the formulæ reduce to

(23) The deflection of plank in inches, $d = \frac{45wl^4}{2Eh^3}$.

(24) The weight per square foot of floor in pounds, $w = \frac{2Edh^3}{45l^4}$.

(25) The thickness of plank in inches, $h = \sqrt[3]{\frac{45wl^4}{2Ed}}$.

A floor is much more rigid, and the material is placed to better advantage, when the floor plank are twice as long as the distance between centres of beams, and the mechanics of the plank are those of a uniformly loaded, rectangular beam, supported at one end and fixed at the other. Its deflection † is represented by

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* The modulus of elasticity is most conveniently measured by observations upon a beam supported at both ends, and loaded at the centre, $E = \frac{432\,Wl^3}{bh^3d}$.

For heavy loads on narrow beams care should be taken to measure any sinking of the whole beam caused by slight crushing at the points of support. The deflection due to the weight of the beam is a constant quantity, which should be deducted from measurements of total deflection; that is, the deflection of the centre of the beam by its own weight, before any loads are applied, should be taken as the starting-point.

† The formula for deflection is obtained by substituting in $\frac{d^2y}{dx^2} = \frac{M}{EI}$

Let P = upward force at supported end, $M = Px - \frac{wsx^2}{2}$

$$\frac{d^2y}{dx^2} = \frac{1}{EI}\left(Px - \frac{wsx^2}{2}\right)$$

$$(26)\ d = \frac{432wsl^4}{Ebh^3}\left(\frac{2x^4}{l^4} - \frac{3x^3}{l^3} + \frac{x}{l}\right)$$

$$\frac{dy}{dx} = \frac{1}{EI}\int\left(Px - \frac{wsx^2}{2}\right)dx$$

$$\frac{dy}{dx} = \frac{1}{EI}\left\{\frac{Px^2}{2} - \frac{wsx^3}{6}\right\} + C.$$

The plank being horizontal at fixed end where $x = l$ and tangent to angle of depression $\frac{dy}{dx} = o$.

$$C = -\frac{1}{EI}\left(\frac{Pl^2}{2} - \frac{wsl^3}{6}\right) = -\frac{Pl^2}{2EI} + \frac{wsl^3}{6EI}$$

$$\frac{dy}{dx} = \frac{Px^2}{2EI} - \frac{wsx^3}{6EI} - \frac{Pl^2}{2EI} + \frac{wsl^3}{6EI}$$

$$\frac{dy}{dx} = \frac{1}{2EI}\left(Px^2 - \frac{wsx^3}{3} - Pl^2 + \frac{wsl^3}{3}\right)$$

$$y = \frac{1}{2EI}\int\left(Px^2 - \frac{wsx^3}{3} - Pl^2 + \frac{wsl^3}{3}\right)dx.$$

$$y = \frac{1}{2EI}\left\{\frac{Pl^3}{3} - \frac{wsx^4}{12} - Pl^2x + \frac{wsl^3x}{3}\right\} + C.$$

$y = o$ for $x = o$, therefore $C = o$;

also $y = o$ for $x = l$.

$$C = \frac{Pl^3}{3} - \frac{wsl^4}{12} - Pl^3 + \frac{wsl^4}{3} = o.$$

$$P = \frac{3wsl}{8}$$

substituting this value of P in first equation.

$$\frac{d^2y}{dx^2} = \frac{1}{EI}\left(\frac{3wslx}{8} - \frac{wsx^2}{2}\right)$$

$$\frac{dy}{dx} = \frac{1}{EI}\int\left(\frac{3wslx}{8} - \frac{wsx^2}{2}\right)dx$$

$$\frac{dy}{dx} = \frac{1}{EI}\left\{\frac{3wslx^2}{16} - \frac{wsx^3}{6}\right\} + C.$$

$$\frac{dy}{dx} = o \text{ for } x = l.$$

$$C = -\frac{1}{EI}\left(\frac{3wsl^3}{16} - \frac{wsl^3}{6}\right) = -\frac{wsl^3}{48EI}$$

$$\frac{dy}{dx} = -\frac{3wslx^2}{16EI} + \frac{wsx^3}{6EI} + \frac{wsl^3}{48EI}$$

$$\frac{dy}{dx} = \frac{wsl^4}{48EI}\left(-\frac{9x^2}{l^3} + \frac{8x^3}{l^4} + \frac{1}{l}\right)$$

At the centre of the span the deflection found by substituting $x = \frac{l}{2}$

$$d = \frac{108wsl^4}{Ebh^3}$$

The maximum deflection * is not at the centre, but 0.4215 of the distance from the supported to the fixed end.

$$d\,(\text{maximum}) = \frac{112wsl^4}{Ebh^3}.$$

To apply this to the load per square foot of floor, substitute twelve inches for b, and s the breadth of the load, equal one foot.

(27) The deflection in inches $d = \frac{28wl^4}{3Eh^3}$.

(28) Weight per square foot of floor in pounds. $w = \frac{3Edh^3}{28l^4}$

(29) Thickness of floor plank in inches,

$$h = \sqrt[3]{\frac{28wl^4}{3Ed}}$$

The support at the fixed ends of the floor plank, under these conditions, sustains five-eighths of the load, and that at the supported end three-eighths.

$$y = \frac{wsl^4}{48EI}\int\left(-\frac{9x^2}{l^3} + \frac{8x^3}{l^4} + \frac{1}{l}\right)dx$$

$$y = \frac{wsl^4}{48EI}\left\{-\frac{3x^3}{l^3} + \frac{2x^4}{l^4} + \frac{x}{l}\right\} + C.$$

$y = o$ for $x = o$, therefore $C = o$.

Substituting the values $I = \frac{bh^3}{12}$ and expressing l in feet, we obtain the equation

$$y = \frac{432wsl^4}{Ebh^3}\left(\frac{2x^4}{l^4} - \frac{3x^3}{l^3} + \frac{x}{l}\right)$$

---

* The maximum value of d is obtained by making

$$\frac{dy}{dx} = o = \frac{ws}{48EI}8x^3 - 9lx^2 + l^3.$$

$$x = \frac{l \pm \sqrt{33l}}{16} = .4215l$$

$$y = \frac{112wsl^4}{Ebh^3}$$

If the plank of a mill floor were placed in uniform rows across the building three-fifths of the load would come upon every alternate beam ; by alternating the joints between the ends of the floor plank every few feet each beam sustains an equal load.

CONSTRUCTION OF FLOORS.

Textile machinery is generally adapted to the floor space which is furnished by placing the beams and columns eight feet between centres, and the beams with twenty-four or twenty-five feet span. In some classes of hosiery and woollen mills a slightly greater distance between beams is an advantage. The mill with floor beams twenty-five feet long and eight feet between centres, is here taken as the standard; and this division of clear floor space is equally convenient to nearly every class of industry.

The top floor should be hard wood, and both floor plank at least two inches in thickness to furnish suitable material to bolt the machinery securely in place.

There are two methods of constructing floors to fulfill the conditions which have been stated with such detail in the preceding pages.

With the beams extending across the mill, the older arrangement consists in placing three by twelve inch joists about twenty inches between centres, with the ends resting upon the beams; over these floors is a lower floor of inch spruce, covered by a top floor of inch and a quarter hard wood; these joists are ceiled underneath with tongued and grooved pine sheathing.

The later system of flooring consists in placing across the beams three-inch spruce plank, planed on the under side, with grooves in both edges, which are filled with splines of hard wood; the plank are rabbeted on the under side and mouldings (beads) nailed into the grooves which are formed by each pair of rabbets, all the nails being driven into the plank on one side of the beads, so that the shrinking of the plank is not seen, nor will such contraction split the beads, as would be the case if they were nailed alternately to each side.

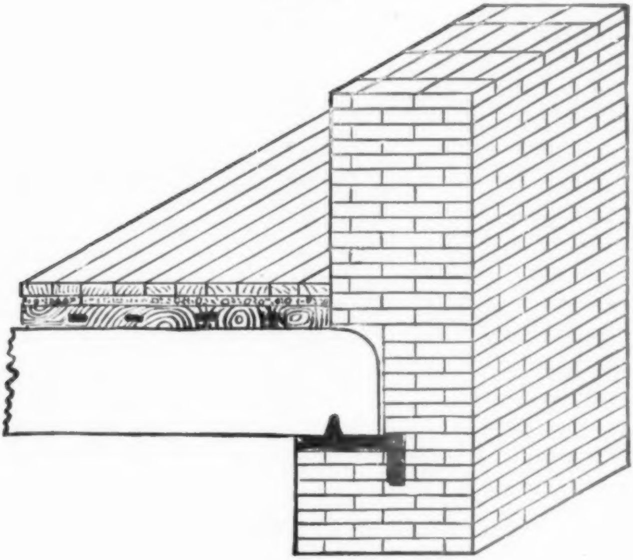
This under floor is covered by a top floor of hard wood one and a quarter inches thick.

A floor should be rendered water-tight by three-fourths of an inch of mortar between the upper and lower floors. The layer of mortar preserves the lumber from decay, prevents the floor from becoming soaked with oil, and is so slow-burning that it is more

nearly fire-proof than any other practicable method of construction.

The mortar can be floated to a uniform thickness by narrow strips (furrs), fastened about eighteen inches apart upon the under

FIG. 148.



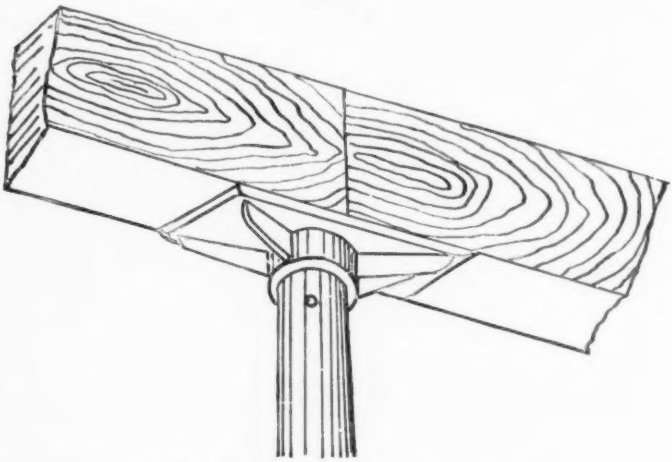
floor. The doorways should be furnished with suitable thresholds, and all belt-holes in the floor provided with iron belt-guards, which reach through the mortise, as well as extending above the surface of the floor.

FIG. 149.

The beams should rest on cast-iron caps at the columns, each vertical line of columns provided with iron pintles, connecting the cap of one with the base plate of the others, so that the ends of the beams will not be crushed by the accumulated weight sustained by the columns above. The ends of the pintles and the iron plates against which they rest should be turned true, so that the contact will be uniform.

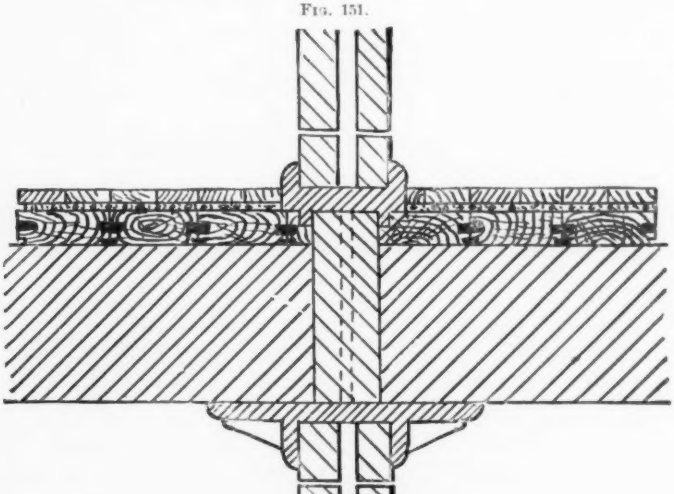
Floor beams should not be bolted through the walls; it adds but little to the stability of the structure, and there is great lia-

FIG. 150.



bility in case of fire that the falling beams will overturn the walls. The beam is securely bound to the walls, without this

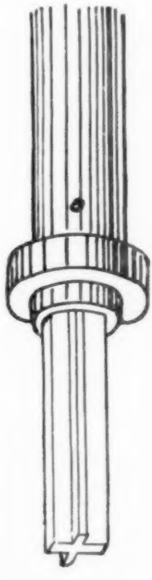
FIG. 151.



ulterior fire hazard, by imbedding in the masonry a flat cast-iron plate with a transverse fin upon each side near the end, one to

secure the plate in the wall and the other fitting in a groove across the under side of the beam. The bricks in the wall for about five courses above the beam should be laid dry, and the upper edge of the beam at the end slightly rounded. When such a beam is in position it is securely held to the wall; if broken by fire, the bending at the centre will lift the extreme end sufficiently to clear the iron ridge, permitting the beam to slide from the plate without injury to the wall.

FIG. 152.



Of these two methods of mill-floor construction the solid one is preferable in the following respects:

Cost.—The hollow floor requires the same amount of southern pine, the same number of feet of spruce, but cut to different dimensions, and in addition, pine sheathing, amounting to seven-eighths the area of the floor. The hollow floor being ten inches thicker than the solid floor, it requires ten inches more wall to each story, and a similar increase in stairways, all pipe connections, and belting from one story to another. The additional quantity of masonry required in a building with the stories twelve feet in the clear is seven per cent. more where a hollow floor is used than where a solid floor is used.

Proportions.—The solid floor is better proportioned to resist

deflection, in a uniform manner, both of the beams and of the floor plank. The weights of each part of these two classes of floors are.

	Pounds Per Square Foot of Floor.	
	Hollow Floor.	Solid Floor.
Southern Pine, 1¼ inches, ...	5.00	5.00
Mortar, ¾ inches, ...	6.25	6.25
Spruce, 1 inch, ...	2.50	
" 3 inches, ...	. . .	7.50
" Joist, 3 by 12 inches, ...	5.03	
Pine Sheathing, ⅞ inch, ...	1.93	
Weight of Floor on Beams, ...	20.71	18.75
Southern Pine Beam, 12 by 14 inches, . . .	6.85	6.85
Total Weight of Floor, ...	27.56	25.60

Although the quantity of lumber is nearly identical in each instance, yet in the case of the hollow floor the floor boards, mortar, and sheathing are dead loads, amounting to 15.68 pounds to the square foot of floor, and being laid across the joists, they do not assist the joists in supporting the whole load, while in the solid floor the mortar, weighing 6.25 pounds to the square foot, is the only element which does not assist in sustaining the load, although the stiffness of the upper floor is so slight that it is omitted from the computations.

From the table of weights of cotton machinery, operatives, and stock in process, the heaviest load in the main mill is the frame-spinning room, where the load upon the floor is 31.42 pounds to the square foot. Including the weight of floor, 60 pounds may be taken as the representative load for the beams, and 53 pounds for the floor plank of the solid floor, or the joists in the hollow floor.

Resuming the consideration of the curvature of the floor, to ascertain the flexure of the floor beams in a chord of 8 feet. The size of the beam is given 12 by 14 inches, and 25 feet span. The deflection at various points in the beam is found by substituting corresponding values of x in (19)

$$d = \frac{864wsl^4}{Ebh^3}\left[\frac{x^4}{l^4} - \frac{2x^3}{l^3} + \frac{x}{l}\right]$$

Distance from support. Feet.	Deflection at given points. Inches.	Deflection in chord of 8 feet. Inches.
0	.0000	
4	.3852	.059
8	.6519	.074
8.5	.6760	
12	.7672	
12.5	.7675	.091
Mean versed sine,		.075

.075 inches is one-twelve-hundred-and-eightieth, .00078, of a chord 8 feet long, and substantially agrees with the limit previously taken for the distortion of the floor. Applying this limit .075 to the floor plank, substituting in (29)

$$h = \sqrt[3]{\frac{28wl^4}{3Ed}} = 2.82 \text{ inches}$$

for thickness of plank, and the nearest commercial size is the 3-inch plank used for this purpose. If the floor plank were cut 8 feet long, instead of 16 feet, the necessary thickness would be found by substituting in (25)

$$h = \sqrt[3]{\frac{45wl^4}{2Ed}} = 3.79 \text{ inches.}$$

The nearest size being four inches, and shows a saving of twenty-five per cent. by using three-inch plank 16 feet long, instead of 4-inch plank 8 feet long.

The corresponding deflection in a hollow floor built in the conventional fashion is found by applying (20)

$$d = \frac{270wsl^4}{Ebh^3} = .016 \text{ inches,}$$

or one-six-thousandth of the span—a rigidity which is unnecessary, out of proportion to that of the beams, and involving a waste of material.

For the proper height of floor joist for deflection .075 inches, apply (22)

$$h = \sqrt[3]{\frac{270wsl^4}{Ebd}} = 7.11 \text{ inches,}$$

or about sixty per cent. of the lumber generally used for such joists.

Another advantage of solid floors is in the convenience with which belt-holes can be cut in any place between beams, while a hollow floor is frequently weakened by joists cut away at belt-holes.

A hollow floor merely resting on beams is much more susceptible to vibration than a solid floor with the plank spiked securely upon the beams.

The concealed spaces between the joists are a lurking-place for vermin, rats, and mice, adding to the fire risk by collecting oily waste for their nests. There is no fire apparatus adequate to extinguish a fire once well under way in such concealed places. If there is no ceiling underneath, the projecting corners of the joists serve as kindlings to extend fires which start in the room below.

Notwithstanding its defects the hollow floor is the mill floor in general use outside of a relatively small limit.

The writer has endeavored to show that a solid floor is the preferable one for industrial purposes in every particular of economy, design, convenience, and safety.

In reference to the remarks in the President's address relative to the use of electricity for the transmission of power, it has been proposed to build mills one story high on level land near water privileges, and by means of electricity transmit the energy of the falling water to the mill. Of course this is only a suggestion which has not yet been carried out; but it has received serious consideration, and only waits for the application of the electrician's skill to be carried into effect.

DISCUSSION.

THE PRESIDENT: There are one or two points which occurred to me during the reading of the paper. If Mr. Woodbury will allow me, I would like to suggest that the experiments on the peculiar behavior of wood exposed to strains after long periods of time, an account of which was published in the *Journal of the Franklin Institute*, should be credited not to me, but to the Mechanical Laboratory of the Stevens Institute of Technology. The work was done during my absence. It was arranged and performed by Mr. Denton, who was then my senior assistant. I had nothing to do with it, except with the method of publication. The plan was that gentleman's and the results were entirely his.

Referring again to Mr. Woodbury's paper, I am reminded of a case that came up in my practice some time ago. It was proposed

to establish a foundry at a place where conflagration would be particularly disastrous should it occur. Over the rooms devoted to the foundry was a double floor—an old-fashioned double floor. I had the lower lining of the floor—the ceiling of the room below—all torn away and the floor above taken up, and a set of laths bevelled in such a way that when nailed in place a space would be left for mortar. The laths presented a dovetailed section and were all plastered together. I presume that the arrangement of that floor is as fire-proof as any so-called fire-proof floor. I think Mr. Woodbury's suggestion is a very good one.

Professor Trowbridge: I would like to ask whether these vibrations act to the extent of producing appreciable detriment to the mill. If the vibrations are not sufficient to do any appreciable injury, then they belong to that peculiar class of phenomena which do not deserve to have much importance attached to them. Immediately after the construction of the Derby dam, almost every house in the city of New Haven, ten miles away, was occasionally subject to gentle but perceptible vibrations, and these were evidently due to the water falling over that dam. If the vibrations are of sufficient extent to affect sensibly the working of the machinery and the quality of the products, the investigations have great importance.

Mr. C. J. H. Woodbury: To answer this question involves the difficult task of expressing well-known facts of mechanics in terms of dollars and cents. The detriment which vibration entails upon production is secondary in its nature, because the injury is first wrought upon the machinery and the cost of power. Any distortion of a machine or looseness of joints caused by the additional wear from vibration certainly injures the operation of that machine to the same extent that precise work is required of it. As examples of the injury caused by vibration, the fluted rolls on the front of a spinning-frame, are made in lengths of about three feet, and are keyed together in one continuous roll extending the length of the frame by square dowels which fit into square sockets in the ends. When these connections become loose by continuous vibration, the irregular motion of the front rolls makes the yarn uneven. Some printing presses were placed on a floor which was deflected by their weight; after the presses were brought to a level bearing by wedges driven under some of the feet, the presses would not operate satisfactorily, the upward components of vibration being excessive, and it was necessary to support the presses

independent of the floors by pillars built up from the basement. It is well known that all rapid heavy machinery, as cotton pickers, planing machines, and circular saws, must rest upon solid foundations. Some idea may be obtained of the precise operations necessary in the manufacture of cotton, when we consider the cotton manufacture of the United States is spun between the limits of number 13 for sheetings and number 80 for lawns; coarser and finer work is exceptional. Number 13 weighs five-eighths of a grain to the yard, and number 80 weighs one-tenth of a grain to the yard. This difference of half a grain to the yard of yarn represents the whole difference in American cloth; and in the spinning of any yarn, the allowable deviation from the standard is expressed in thousandths of a grain to the yard. In order to produce any line of goods, the cotton must be measured and weighed at every stage in the process, and the machinery kept adjusted to the varying conditions of atmosphere, temperature, and raw material. After a consideration of the facts, it requires no argument to prove that it is more expensive to operate a mill both in regard to power, repairs, and product, when everything is bending to and fro, making alternate strains at every bearing, distorting and straining the machinery and swaying the whole building, than it is to operate a mill when the whole structure and its contents is a rigid mass.

LIII.

A SELF-PACKING VALVE.

BY H. F. J. PORTER, TRENTON, N. J.

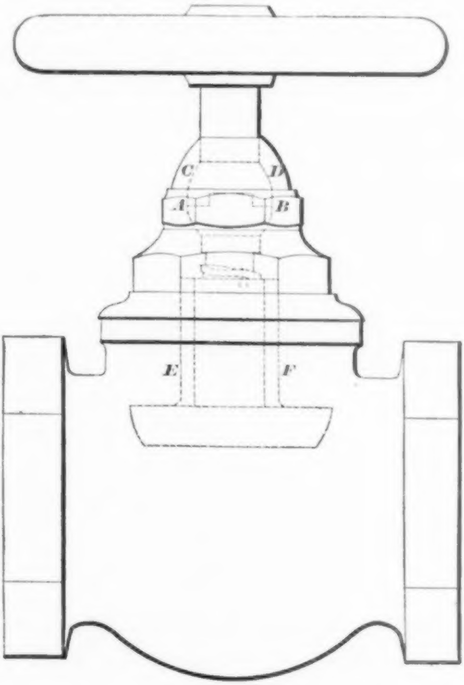
I HAVE deemed it almost a duty to present to the attention of the Society an improvement in the usual form of steam valve, which, as far as the packing is concerned, brings it, in my opinion, almost, if not quite, to a state of perfection.

It will not be necessary to call to the attention of an engineer the constant source of annoyance and expense due to the leakage of steam about the stem of the present market valve. That this defect has received great consideration during the past few years is amply proven by the number of patents issued to the inventors of various kinds of packing, each claiming to be superior to all other combinations, and yet none accomplishing the desired result, namely, a permanently tight stem.

In factories and large buildings where steam is used, either as a motor or a heater, it has ever been an item of great expense and trouble to repack the valves and repair damages caused by leaking stems, amounting in many cases to more than the original cost of the valves in one year.

It was my fortune during the last summer to learn of a number of these self-packing valves, which had been in constant use in the works and mines of the Lehigh Zinc Company, Bethlehem, Pa., for the past two years.

FIG. 153.



Some of these were used as throttle valves on various rock drills, and these received probably as thorough a test as could have been given them, being constantly under the hand of the engineer. The latter, however, on being questioned as to their action, replied that they had "never wept a drop."

Such being the case, it is indeed time to call the attention of this Society to its construction, that it may pass judgment upon its merits, as well as its defects.

In external appearance, as seen from the sketch, it does not

differ materially from the present market valve, except that it is possibly a little more compact. Its beauty of design lies in the internal arrangement, which is such as to entirely dispense with packing of any description.

A short explanation of the sketch will lay the matter before you.

As seen from the dotted lines, the stem is divided at the line *A B*, and the two parts connected by a device similar to an ordinary clutch coupling.

The interior of this coupling is a hollow cylinder, in which is placed a common helical spring, having for its function the keeping of the ball valve (*C D*) upon its seat in the cap. The lower portion of the stem is furnished with a square thread, whose duty is to raise and lower the main valve. The main valve stem is cylindrical, with two **V** lugs upon it, moving in corresponding **V** ways in the fitting. These ways allow the valve sufficient motion to always find its seat properly, and yet prevent any turning upon the seat, and thus alleviating the cutting and tearing, so common in ordinary valves when grit or iron rust sticks upon the seat.

I have taken pains to inquire wherever I could find that these valves have been put to practical test, and find that they have gained universal sanction.

Considering the defects of the ordinary valve, and the fact that these are completely overcome by this self-packing valve, I feel sure that engineers in general will indorse this as an improvement in the present form of steam valve.

DISCUSSION.

MR. PIERCE: I hardly deem it in place for me to explain the action of that valve, the tests of which I had under my supervision long before I took any interest in the actual manufacture of valves. I am now united with some others in the manufacture of the article, and we shall be ready to place it in the market in some few weeks. I think I had better find fault with it than praise it. In the words of one of my contemporaries, Mr. Woodbury, all valves should be excluded which have not the advancing stem. The valve you see before you has no advancing stem. We think it one of the beauties of the valve that the stem itself is stationary, and the ordinary packing, if there is any, would not be cut away. In our case we have a ball-valve and a seat. The

ball portion of the auxiliary valve, which, as you can see from the dotted line, is joined to the lower portion of the stem by means of an ordinary clutch, is hollow, and contains a helical spring. This helical spring keeps the upper portion of the stem always on its seat. That prevents grit of any kind getting between the surfaces, and has thus far, during the past three years and over, prevented any cutting of the seat which would cause leakage. In fact, I know of none of the valves which have been under my supervision and been submitted to the most severe tests, which have leaked at that point. The main valve, which has its V ways upon the cylinder stem of V lugs, is of course, as Mr. Porter stated, prevented from any rotary motion, but it has about sufficient play in the fitting to take its seat properly. This prevention of rotation of course is accomplished in the market valve of to-day to a great extent by the ordinary cylindrical hole in the disk, which allows the disk free motion on the stem in the larger sizes. In the smaller sizes this is not the case. But in the larger sizes, even when the disk comes upon its seat, any portion of grit may hold it there long enough for the stem to be forced down, and it will then twist that disk by friction, as is seen from the annular cutting which will be found in any valve disk. The great objection made by Mr. Woodbury to the non-advancing stem was the fact that it would not be known whether the valve was opening or closing; as some valves are made with left-hand threads and some with right-hand threads. It has been my misfortune never to meet with a valve which opened with a right-hand thread. Any mechanic or machinist will naturally close a valve with the right-hand motion. Of course we may admit as one of the objections against this valve, that when a person was rushing to open or close a valve, that he might open instead of close it, or close instead of open it. But any one who has had much experience about a shop knows that a man turns the wheel, and then watches the stem to see whether it is advancing or receding.

Mr. S. W. Webber: Having at one time been connected with a company that manufactured many thousand valves in the course of the year, and having some experience in selling them, I can perhaps give a few facts in relation to the direction of the threads in the matter of stems.

In many cities and towns they use valves with the left-hand thread almost exclusively in the gas-mains. In the City of Boston, for instance, all the gas-valves turn to the left. The objec-

tion made to the rising stems is that they are frequently broken. The company that I was with at the time manufactured both kinds of valves, some with rising stems and some with stationary stems. The objection made to the stationary stem was that you could not tell whether the valve was opened or closed; and oftentimes, particularly where they were on water-mains, the valve would be closed, and stuck to the seat frequently, and in their hurry to open them they would first twist to the right and then to the left, and generally ended by breaking off the stem and rendering the valve entirely useless. And there were, of course, many objections made to having valves in which you could not tell in what position the valve was, whether open or shut. A great many valves are overheated and hard to get out. There were times when it was desirable to tell whether the valve was open or shut by simply looking at it. I think that the majority of valve-users prefer stems that run in and out, so that they can tell whether the valve is open or closed, or how much open. Particularly in the case of valves used for fire purposes, it is very desirable that the stems should all turn one way, and should run out, and there are many valves, many more than one would think, that are made with the left-hand thread. As I say, nearly all gas-valves are made in that way.

A MEMBER: As I understand it, the auxiliary valve which makes the valve tight is loose on the stem.

MR. PIERCE: It makes a portion of the stem, inasmuch as it is a continuation of the stem by the ordinary clutch, and is kept upon its own seat by means of the helical spring which is contained within the large portion of the stem. The stem with the wheel on is the stationary portion. The lower portion of the stem has a shoulder on it corresponding to the upper portion. That seating in the fitting of course prevents the lower portion of the stem from any motion except that of rotation.

LIV.

THE LIFETIME OR AGE OF STEAM-BOILERS.

BY WILLIAM BARNET LE VAN, PHILADELPHIA, PA.

THE advisability of imposing a limit to the age of a steam-boiler under pressure is well illustrated by the explosion of a steam-boiler at the rolling-mill of Messrs. J. Wood & Brothers, at

Conshohocken, Pa., on February 3d, 1873, resulting in the killing of eleven and the wounding of twelve persons, beside causing great destruction of property.

The above works are situated on the west side of the Reading Railroad Company's track, just below the bridge which crosses the same, and alongside of the canal, which runs parallel to the railroad. The building covers a space of one hundred by two hundred feet. About one-half the works were destroyed by the accident, the upper end being a complete ruin, in which sections of the roof of the destroyed buildings, bricks, pieces of iron-plates, twisted rods, and bent bolts, lay in one confused mass.

There were in the mill seven boilers, each being separate and distinct. The one that burst was at the northern end of the building, and its course when projected by the explosion was directly west across the canal.

The exploded boiler had been in use for nearly twenty years. When the rupture took place it was lifted from its settings, and shot across the canal like a projectile from a cannon, at about the same height as its original position, until it reached the building, 150 feet distant, on the other side of the canal, known as the Albion Print Works. This immense mass, weighing about 5500 pounds, struck the end of a girder twelve inches square over an arched doorway, shattering the same and tearing away the wall, and finally brought up against a largely cylindrical vessel of wrought iron, eight feet in diameter, twelve feet high, and seven-sixteenths of an inch in thickness, called a "keir," in which at the time were two lads, George Smith and James McNulty, who were arranging the pieces of endless muslin within, by tramping them down so that the steam and limewater which were to be subsequently let in would not mix up and entangle the material. The only mode of ingress or egress was a small manhole on the top of the vessel. The two poor little fellows were tramping away when the end of the flying boiler came in contact with the keir. The heavy iron sides gave way before the shock as if made of wetted paper; the indentations nearly reaching the opposite side, instantly killing both lads, George Smith being actually cut in two. The force and impact of the blow of the flying boiler was so great that the piece of the wooden girder which it carried with it was ground into splinters, and the atoms, being packed so closely together, were ignited by the friction and burst into flames, setting fire to the material contained in the keir.

Nothing was left of the exploded boiler at its original location except about two feet of one of the flues, the mud-drum, and two-thirds of the back head.

The balance, consisting of the shell containing the two flues, lodged as above stated in two, as per following cut:

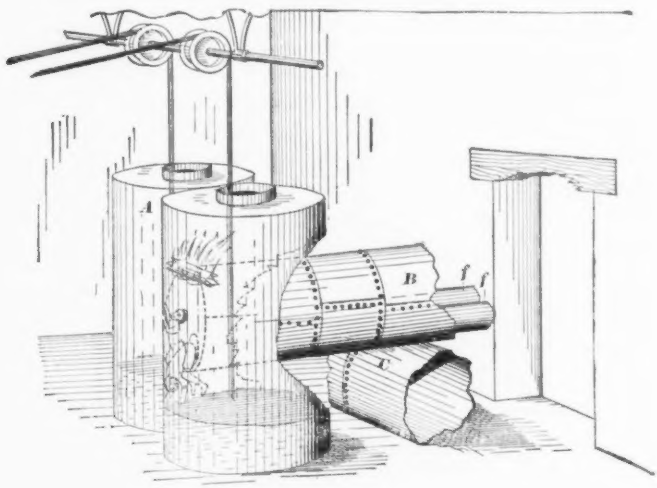


FIG. 154.

A, represents the keir.
B " shell of boiler, containing the two flues, about fourteen feet long each.
C, " shell of boiler, about four feet long, open on both ends.
f, f, " the flues.

Shell B represents the boiler with one end resting in the indentation of the keir, the other end supported by a piece of the shell C, about 4 feet long and 54 inches in diameter, at right angles to the shell B.

After the boiler was removed, the hole in the side of the keir was found to be about three feet in diameter.

DESCRIPTION OF THE BOILER.

The boiler consisted of a cylindrical shell, 18 feet in length, and 54 inches in diameter, and containing in its lower half two flues, each 18 feet long and 16 inches in external diameter, secured to the heads with angle iron rings. To the bottom, about three

feet from the back end, was attached a mud-drum, 18 inches in diameter, 7 feet in length, by a pipe 6 inches in diameter and 24 inches long, into which the feed-water was pumped. It was set, in the usual way, over the heating furnace, the waste heat of the latter passing underneath the shell, thence through the flues to a wrought-iron stack placed over the front end.

The following are its principal dimensions and proportions, namely:

Extreme length, in feet,	18
Diameter of shell, in inches,	56
Number of flues,	2
Extreme length, in feet,	18
External diameter of flues, in inches,	17
Extreme length of mud-drum, in feet,	7
External diameter of mud-drum, in inches,	18
Thickness of iron in shell, in inches,	0.257 = No. 2
Thickness of iron in flue, in inches,	0.257 = No. 2
Thickness of iron in heads, in inches,	0.454 = No. 0000

The safety valve was $3\frac{1}{2}$ inches in diameter, and was in good order when found after the explosion, as also was the three-inch stop valve, the latter being closed. This valve was placed between the boiler and the main line of steam-pipe which conveyed the steam from all the boilers to the engine.

The flues at the point of rupture were but three-sixteenths of an inch in thickness, and were as clean cut as if done with a pair of shears, allowing a ring of metal remaining in the angle iron ring which secured the flues to the head as though it was so ordered. The flues were flattened together as though they were passed through a pair of rollers.

The iron composing the flue plates was very much crystallized at the point of parting, no doubt caused by the constant expansion and contraction, and there is no doubt but that a certain amount of this crystallization was due to the use of angle iron rings to connect the flues to the heads; the flue plates would not flange from being *hot short* iron.

The boiler had been out of use for a few days for repairs, and they were in the act of getting up steam at the time of the explosion.

TESTIMONY BEFORE THE CORONER'S JURY.

John Welsh testified "that he is a boilermaker; last week repared the boiler that exploded; he considered the boiler in *good and safe condition* when he left it."

John Mellon, a laborer in the mill, also testified "that he made a fire under the boiler about two o'clock on the day it exploded; had then *tried the safety valve*, and did not think it could have been *stuck*."

William T. Bate, boilermaker by trade, examined the boiler carefully in the presence of Coroner Strahley. "In some places he found it *three-sixteenth* full, and in other places *three-sixteenth scant;* did not think the plates thinned any in the explosion." He also "found the iron in the bottom of the shell a *scant quarter of an inch;* . . . he wouldn't like to work much around a boiler of that diameter and thickness."

The Engineer's Statement.

"The engineer testified that he had observed the fireman firing up at about two o'clock in the afternoon; the blast was put on the furnace at half-past two, another man having succeeded the first fireman; the explosion occurred at twenty minutes past four, the blast having been on the furnace until that time; there was no unusual forcing of the fires; witness had in the meantime tried the gage-cock and water-gage; usually carry about from eighty to eighty-five pounds steam-pressure on the gage; had about *fifty pounds* pressure about ten minutes before the explosion; never knew the safety-valve to stick; carried the weight on the lever of the safety-valve at about eighty-five pounds, at which pressure the valve would blow off; noticed nothing unusual about the boiler; there had been no water in the boiler until the day on which the explosion took place; he (engineer) pumped the water into the boiler right out of the river; there was no water in the boiler until half-past twelve on the day of the explosion; it generally took about three and a half hours to generate steam enough to run; the man who took the place of the one that fired up in the first place is among the killed; the boiler was nearly full of water; *it blowed water from the top gage-cock;* never was in the habit of hanging any weights on the safety-valve lever."

In answer to a question of a juryman, the engineer said that "to the best of his knowledge, there was not more than *fifty-five or sixty* pounds of steam-pressure when the boiler exploded."

Statement of the Owners.

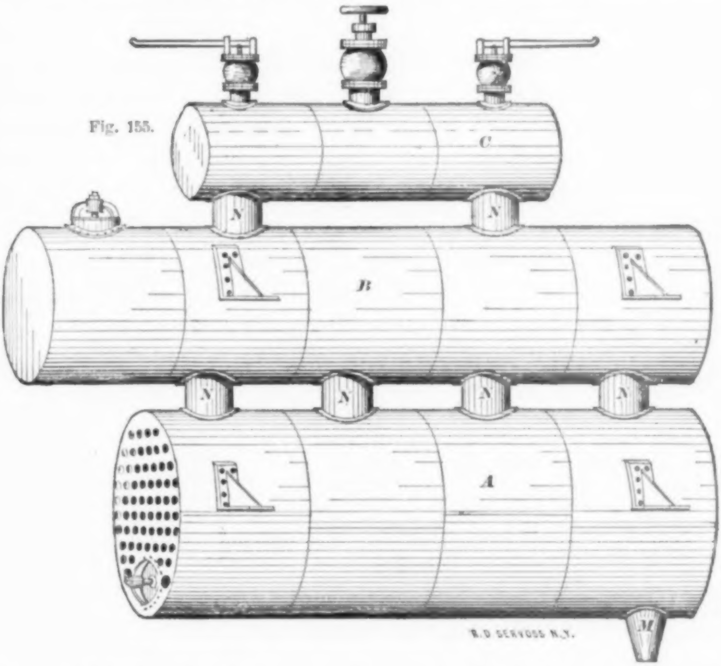
The owners stated that "*it was thoroughly repaired and inspected just before starting it, and pronounced all right.*"

And also that "*it carried eighty-five pounds steam-pressure per square inch* before it was repaired, *without showing the least strain whatever!*" Nevertheless this boiler *exploded with less* than *eighty-five pounds steam-pressure per square inch.*

The Verdict of the Jury.

"That the deceased (names mentioned) came to their deaths by the explosion of a boiler in the rolling-mill of J. Wood & Brother, in the above borough (Conshohocken), and, in the opinion of the

Fig. 155.



jury, said boiler had *by long and continued use* become, in certain parts, inadequate to carry the required pressure, viz., eighty pounds."

From the above it will be seen that this boiler was *twenty years old* and had been in use for that entire period, and the iron at the point of rupture was *but three-sixteenth* (0.1875) *of an inch thick*, and when new, in 1853, it was *full one-fourth* (+0.257) *of an inch thick.*

The reduction by *wearing away from age in twenty years* was *full one-sixteenth* (−0.0695) *of an inch.*

Other parts of the boiler showed in places *a reduction by wearing away by age*, of nearly *five-thirty-seconds* (0.15625).

Yet the owners of this steam-boiler stated it was thoroughly repaired and inspected as before remarked.

In a more recent case the boiler which exploded was under the care of trained inspectors, who made boiler inspection a specialty. I have reference to the Wilt & Son steam-boiler, which exploded in Philadelphia, June 27th, 1879. This boiler was built and put in operation about the beginning of 1870, and, as before stated, exploded on Friday, June 27th, 1879, after being in use but *nine years*.

It was built by a house of long standing and high reputation in the city of Philadelphia, and was inspected, insured, and under the care of the Hartford Steam-Boiler Inspection and Insurance Company.

DESCRIPTION OF WILT & SON'S BOILER.

The boiler is known as a "two story," sometimes called "double decker," in fact this boiler was three stories, as it consisted of three cylinders placed parallel one above the other, and connected by necks made of boiler-plate, see Fig. 2.

The principal dimensions are as follows:

Lower Cylinder A.

Length, in feet,	12
Diameter, in inches,	54
Diameter of flues,	71
Diameter of flues, in inches,	3.5
Length of flues, in feet,	12
Thickness of shell plate, in inches,	0.34
Thickness of heads, in inches,	0.3648

Middle Cylinder B.

Length, in feet,	14.5
Diameter, in inches,	42
Thickness of plates, in inches,	0.2
Thickness of heads, in inches,	0.3648

Upper Cylinder or Steam-drum.

Length, in feet,	8
Diameter, in inches,	24
Thickness of plates,	0.25

The lower and upper cylinders are connected by four wrought-iron necks, N, 12 inches in diameter, and the middle and upper cylinder by two wrought-iron necks, N, 0.29 thick.

The furnace was placed under the front end of the lower cylinder, which was filled with flues $3\frac{1}{2}$ inches in diameter; the products of combustion passed under it to the back end, then returned through the flues to the front end, and thence passed by a brick flue inclosing the four necks and the top of the lower cylinder and the bottom of second cylinder, to the chimney in the rear of the boiler.

Messrs. Corbin & Goodrich, agents of the Hartford Boiler Insurance Company, say in a card issued to the public after the explosion, dated July 19th, 1879: "In January, 1879, a thorough inspection of this boiler was made. One sheet found to be blistered was replaced. A slight blister on another sheet was cut away, as is the custom in all such cases when the strength of the iron is not materially impaired; and the fact that the rupture, at the time of the explosion, did not touch this so-called defective portion, proves that the judgment of our inspector was not at fault. New tubes were put in, and *the boiler placed in first-class condition in every respect. No defects impairing the strength of the boiler existed in any sheet that human inspection could discover, as the sheets show for themselves to-day. Had any defects been apparent, it is reasonable to suppose that they would have been repaired before the company assumed the liability under their policy of* $5,000."

"The iron was 'C. H. No. 1' iron, made by the best makers, and the boiler was manufactured by Messrs. Jacob Naylor & Co., whose name alone is a guarantee to those who know them that proper material was used in its construction. Tests of the iron from different sheets show unusual strength, both with and across the fibre. *Iron, while in use, will often change its character, and, although tough and fibrous when new, from various causes, such as the constant expansion and contraction of every-day use, etc., will become crystallized and brittle.* Fortunately for the proper determination of this question, *the iron still remains,* and *tests and experiments are now in progress that will fully ventilate the subject as has not been done yet;* and the Hartford Boiler Insurance Company is entirely satisfied to abide by the results that may be developed."

"The Company *spare no expense to secure the best talent for inspectors that the country affords.*" (The italics are my own.)

Messrs. Corbin & Goodrich also said to a reporter of one of the morning papers: "*We considered it in first-class order on the*

14th of last May, tested it, and were perfectly satisfied with its condition."

Dr. Charles Huston, of Lukens's Rolling-mill, according to his report, as published by the Hartford Boiler Inspection and Insurance Company, made trial tests of (18) eighteen pieces *said* to have been taken from this Wilt & Son steam-boiler, in order to ascertain the tensile strength of the iron from which it was constructed.

The results of Dr. Huston's tests were as follows:

Marks.	Width.	Thickness.	Area.	Breaking Weight.	Tensile strength to sqr. inch.	Contraction of Area.	Remarks.
AA–LGT	.770	.354	.2725	14290	52440	17.4	Fracture fibrous, with a few crystals.
AA–LGT	.817	.355	.2900	15000	51720	19.0	" fibrous, with a few crystals.
A1–LGL	.789	.372	.2935	15625	53230	17.9	" wholly fibrous.
A1–+	.773	.365	.2821	12295	43580	6.2	" wholly fibrous.
A2–LGT	.770	.382	.2941	14630	49700	19.3	" almost entirely fibrous.
A2–+	.765	.370	.2834	11635	41200	4.2	" wholly fibrous.
B1–LGT	.781	.322	.2514	13725	54596	17.4	" wholly fibrous.
B1–+	.784	.322	.2524	10620	42070	4.5	" wholly fibrous.
B2–LGT	.769	.347	.2668	14300	53450	25.0	" wholly fibrous.
B2–+	.769	.356	.2737	12870	47000	5.0	" fibrous, with very few crystals.
C1–LGT	.798	.350	.2793	14000	50100	12.3	" wholly fibrous.
C1–	.798	.361	.2880	11650	40400	6.0	" wholly fibrous.
C2–LGT	.780	.375	.2925	17250	59000	28.6	" wholly fibrous.
C2–+	.754	.380	.2865	15110	53000	10.0	" wholly fibrous.
D1–LGT	.763	.329	.2510	13025	51890	24.0	" wholly fibrous.
D1–+	.754	.340	.2563	10200	40000	6.8	" wholly fibrous.
D2–LGT	.784	.340	.2665	14850	55700	23.0	" wholly fibrous.
D2–+	.755	.352	.2657	11950	45000	9.0	" wholly fibrous.

LGT indicates specimen cut lengthwise of plate, or "with the grain."
+ indicates specimens cut across the grain.
All the specimens were cut from the old plate except those marked C2, which were from the new plate.

Of nine pieces cut lengthwise from the old plate, the tensile strength (see Table) to the square inch ran from 49,700 to 55,-700, and in seven pieces cut across the grain the tensile strength was from 40,000 to 45,000.

Two pieces from the new plate with which the boiler was patched gave 53,000 across the grain, and 59,000 lengthwise of the plate. All of the above samples except four were noted "fracture wholly fibrous," three "fracture fibrous, with a few crystals," and one "fracture almost entirely fibrous."

Strange to say the elasticity of the iron, which is of the greatest importance, was not stated by Dr. Huston in the report.

The weakest specimen, it will be seen, showed a tensile strength of 40,000 pounds per square inch.

Dr. Huston closes as follows: "*I see no fault to be found with the quality of the iron.*"

These samples no doubt were as represented, but the experiments made by the Chief Boiler Inspector of Philadelphia, Mr. John Overn, with iron taken directly after the explosion from the exploded boiler to the testing machine, gave materially different results, as follows:

Iron from Wilt & Son's, by Riehlé Bros.' Testing Machine.

Marked.	Size in inches.	Area in sq. in.	Broke at pounds.	Strain per sq. in. in pounds.	Limit of elasticity in pounds.	Limit of elasticity per sq. in. in pounds.	Elongation in five inches.	Elongation in per cent. of length.	Area of reduced section per sq. in.	Strain per sq. in. reduced to section in pounds.
Cross grain I	0.989 × 0.338	0.3342	14061	42070	4500	13465	0.07 in 4″	1.4	0.3277	42905
With grain II	1.11 × 0.347	0.3473	19000	54707	8500	24474	0.11 in 4″	2.75	0.3250	58461
" " III	1.10 × 0.338	0.3383	16000	47592	8000	23647	0.10 in 5″	2.5	0.2982	53655
C. grain IV	1.15 × 0.34	0.3404	15500	45534	5000	14688	0.07 in 4″	1.4	0.3270	47094
Blistered V	1.15 × 0.271	0.2714	11500	42409	9000	33161	0.08	2.	0.2545	45186

RIEHLÉ BROS.
(Signed) JOHN OVERN.

Two samples cut lengthwise of the plate, 47.592 and 54.707 pounds respectively, and two from across the grain, 42,070 and 45.534, also one from blistered sheet, 42,409 pounds.

The highest elastic limit was but 2.75 per cent. of length, and ran down as low as 1.4 per cent., the former being from samples cut lengthwise of the plate.

The poor quality of this iron is shown by its narrow limit of elasticity, which corresponds with the rotten fibres shown in the fracture; namely, of a whitish gray color, whilst good fibrous iron shows fracture of a bluish tint.

The iron of the plates along the greater portion of the fractures was partially disintegrated, and in a laminated condition; that is to say, the plates were not solid in one piece, but in layers, like the leaves of a book. This is caused from imperfect welding of the several layers which make up the thickness of the plate, and is due to interposed sand or cinder, which has not been expelled in the hammering and rolling during the process of manufacture.

The best evidence of this is Messrs. Corbin Goodrich's state-

ment above, and also the fact that a large "blister" existed at or near the point at which the rupture no doubt at first commenced; blisters are of a similar nature to laminations arising from the same cause, and are an indication of want of care and skill in the making of the plates.

When a blister is found on the iron of a boiler, if it has a thin side, as was the case on this boiler on the water side, it is chipped off, so as to allow the water to come in contact with the thick side next to the fire.

The flues of this boiler were new, and showed evidence of being very hard when broken, and were entirely crystalline, no signs of fibrous formation appearing at the fracture.

The fact that this boiler failed after having been inspected and *tested* both by *hammer* and *hydraulic pressure*, by men appointed especially for this business, tends to show that as an additional safeguard, a limit to the *age* or *time* that a steam boiler may be allowed to be used, should be imposed *regardless of its condition* at the expiration of such stated period.

Referring to the Old Testament, in Ecclesiastes, third chapter, first and sixth verses, you will find the following:

> "To every *thing there is* a season, and a time to every purpose under the heaven." "A time to *get*, and a time to *lose*; a time to *keep*, and a *time to cast away*."

The *time to cast away* is well understood by our railroad companies, especially the Pennsylvania Railroad, where "no axle is permitted to remain under a passenger car longer than eighteen months;" *it is then condemned and removed, no matter what its apparent condition may be.* I have no doubt but that railroad managers have carefully studied the above quotations, and this settled practice they have found from long experience to be a judicious and economical one.

The repairs to machinery, cars, and roadbed, and the injuries and loss of life caused by defective axles, have emphasized the value of the old adage, "that an ounce of prevention is worth a pound of cure." Iron, like man, *wears away with age*, and should be placed on the retired list, the same as our judges and naval officers.

Referring again to the Old Testament, in the thirty-ninth and ninetieth Psalms, fourth and tenth verses respectively, we find the following:

"Lord, make me to know mine end, and the measure of my days, what it *is;* *that* I may know how frail I *am.*"

"The days of our years *are* threescore years and ten; and if by reason of *strength they be* fourscore years, yet *is* their strength labor and sorrow for it is soon cut off, and we fly away."

The above are not quoted in a theological sense, but to impress upon you that the same ideas existed and have been experienced by the ancients, namely, that the *life of everything is limited.*

The first quotation shows conclusively that we should know our limit of time, so as to look out for the end of our weakness. The second gives the limit of time or age, but admits that we may, for special reasons, as by taking good and proper care of our person, and being temperate and moderate in our own indulgences, exceed that time; but this increased age is soon cut off, and we *fly away, like iron in a boiler if its limit of age is exceeded.*

The lifetime of a boiler depends upon many conditions. Two conditions principally:

First. The thickness of the shell plate over the fire-grate.

Second. The character and extent of the nonconducting *debris* that is formed inside of the shell plate.

The first condition, as to the thickness of the plate, is ordinarily decided by the maximum value of the iron due to the steam pressure per square inch. An excess of thickness of metal over the fire-grate will be practically reduced by the intense heat, from the fact that the fire on the grate-bars being at a temperature of from 1500° to 2000° Fah., it has not, if the metal be too thick, time to communicate its heat to the water in the boiler, the water being a great absorber of heat, and the heat of the fire is expended upon the iron rather than into the water, therefore the shell plate should be as thin as is possible to be consistent with strength. Any excess of thickness of shell plate is as destructive to the economic value of the boiler as insufficient thickness is to its general strength.

From the above it will be seen that the shell plate of a boiler must be neither too thick nor too thin. If too *thick*, the heat will not communicate to the water with sufficient rapidity, and the iron will burn away. If too *thin*, there may not be actual metal enough to resist the steam pressure.

The second condition is, where by the formation of *debris* in the bottom of the boiler directly over the fire—such as mud and calcareous scale, both of which are eminently nonconductors—the

passage of heat is partially arrested through the shell plate, and the result is a feeding of the heat upon the iron of the boilers that leads to destruction, for it must be remembered that the temperature over the fire-grate is very close in its degree of heat to that temperature which will fuse the iron of the shell plate above it.

From the above it will be seen that the thickness of the shell plates fired externally is practically limited in both directions, namely, very thin plates cannot be caulked, nor will they resist the ordinary steam pressure as commercially used for economy, and very thick plates cannot be riveted with the same proportion of strength as that of thinner plates, and are liable to be reduced in thickness by excessive heat.

Experience has shown in practice that five-sixteenths (0.3125) of an inch is the maximum that should be used where the fire comes in contact with the shell plate, so that the steam pressure per square inch will regulate the diameter of the shell.

The above is based on what has been found in practice in the fire-boxes of locomotive boilers after a long series of years of use.

In an experience of over thirty years with steam-boilers I have seen many cases where a slight blow of a hammer has punched a hole through the shell plate, which was found not to exceed one-sixteenth of an inch in thickness, when on the preceding day it had carried *ninety pounds* steam pressure to the square inch. Many such cases might be cited, tending to show how dangerous it is to rely upon inspection in one sense, and how important it is to rely upon the qualified age of the boiler in the other. All intelligent engineers know that constant pressure of any kind produces an alteration in the granulations of iron—that is to say the molecules of iron become in a measure disintegrated and eventually destroyed—an alteration which it is impossible to detect by any known test at present in use; such being the case, beyond dispute, it is clear that however well cared for, at the end of *ten years* a steam boiler must be considerably weaker than when first built.

It is well known that by long use the iron of a steam-boiler becomes weakened by corrosion, which acts unevenly on different kinds of iron and in different parts of the structure, and this deterioration cannot be detected by the ordinary inspection, especially as relating to the elasticity of the iron.

This partial disintegration of the fibres or crystals of the iron

by the high steam-pressures now generally carried, will, independently of other causes of weakness, cause after a lapse of time such a reduction of its strength as to prevent an accurate determination of whether or not it is in a fit condition for use.

It seems to me, therefore, that a law defining the number of years during which a steam-boiler may be used is absolutely necessary for the protection of the public, and especially in view of the fact above set forth relative to the explosion of the boilers of Messrs. J. Wood & Brothers and Wilt & Son.

The condition of these boilers was good as by the representation, in the first instance, the evidence of the owners, and in the second case by a certificate of inspection from the Hartford Steam Boiler Inspection and Insurance Company, whose trained and expert inspectors had *only three months previously carefully examined this steam-boiler inside and outside*, and *applied the hammer test*, and *finally by hydraulic pressure*, and yet both these boilers *did explode with less steam per square inch* than was allowed by the existing laws of the United States.

The two explosions before mentioned inculcate the important lesson, that however well cared for and carefully examined, the plates of a boiler *wear away* and become *thinned*, the *elastic limit of the iron becomes reduced*, and the fibres or crystals are altered by granulation, *none of which elements of weakness can be detected by superficial examinations or prevented by any known means in the state of the art.*

Our leading railroad companies condemn the axles under their passenger cars at the expiration of an eighteen months' run, and why? For the protection of the lives of their passengers, and preventing loss of property by accidents arising from such deterioration of iron from *age* as has just been referred to, and this again shows that a limiting of the duration or age of steam-boilers is needed as a precautionary measure for the protection of life and property from boiler explosions.

The writer does not maintain that a well-made boiler will not, if it is cared for properly, last very much longer than one that is neglected; but it is a rather serious fact that a great many steam-boilers are not well made, and a great many more are managed by ignorant and careless persons, often by boys and men who have no proper appreciation of their responsibilities.

Therefore, I again repeat, that a law defining the number of years a boiler may be used seems to be absolutely demanded for

the better protection of the public, and I am fully satisfied from all the facts that have come to my knowledge in an experience of over thirty years, that *ten years* seems quite long enough for an ordinary boiler to be kept in service; and furthermore, from the fact that our present boiler-inspection system is decidedly defective, and this of itself is an all-sufficient reason why this additional safeguard should be provided.

While I am not disposed to cast any reflections upon the manner of boiler inspection as now made (for as far as it goes it is proper and right), yet steam-boilers are now in use which have been in service over *thirty years*, and which are inclosed nearly on all sides with brickwork, so that only a portion is accessible for examination. Especially is such a state of affairs reprehensible when the boilers are located in densely populated parts of our cities, as is the case in several instances in Philadelphia, to my knowledge, one of these being on one of the most fashionable business avenues, surrounded by palatial residences. I must say there is danger, and every one will agree that while a boiler of this *advanced age* may stand the steam-pressure, it certainly does so only at great *risk*, and I am further certain that the public's greatest safety will be found in boilers of *comparative youthfulness*. In fact, the life of one of our humblest citizens is worth the price of all the steam-boilers in the great cities of either New York or Philadelphia.

DISCUSSION.

MR. F. B. ALLEN: While I think it would be a very desirable matter if there could be any arbitrary way by which the age of safe usage of a boiler might be defined, I cannot understand, practically, how anything of the kind can be done without working a great deal of injury. The safe usage of a boiler, in my mind, is determined by three or four practical considerations: The design of the boiler, the material and workmanship, and the manner in which it is managed. It is unquestionably a fact that new boilers are turned out from our boiler shops utterly unfit for the purposes they are intended for. I have personal knowledge of a number of boilers that have been used for a great many years, and as far as can be determined by tests such as are relied upon by engineers to determine the strength and durability of boilers, the boilers are fit for the purposes for which they are used to-day. Our President pointed out, more than twelve years ago, at the

time of the Westfield investigation, if I mistake not, that to some extent the same principle recognized in ordnance might be employed in regard to steam-boilers; that is, that after a certain length of time they should be condemned. But it seems to me if that rule were put into practice it would, from practical considerations, work a vast amount of injury. I do not think the gentleman who has so ably presented the subject to us has pointed out anything on which we can place greater reliance and more faith than our present system of boiler inspection. It is a very difficult matter to determine the amount of crystallization,—and in all that I say I desire to be understood as speaking in my individual capacity,—it is very difficult to determine the matter of crystallization in boiler-plates when in place. A case came recently under my observation of a boiler which had been used in the city of New York for the past twenty-four years. There was no appreciable reduction by corrosion. The boiler seemingly was in good condition, and it safely withstood the water-pressure test of 120 pounds. Though apparently fit to run for a great many more years, simply as a precautionary measure it was taken out. No defect was revealed in a very painstaking examination, but the boiler was removed simply because it was known to be of an advanced age. A great many of these cases are constantly occurring. I recollect distinctly, within the last year, that some experiments were made at the Watertown Arsenal, with strips of iron cut from a locomotive boiler,—certainly subjected to much harder usage than any stationary boiler, and yet that plate showed a great amount of elasticity. There are so many practical considerations that come in to affect the case that I do not see that we can do any better. If any gentleman has any better plan, I should like to know very much what it is,—any plan by which we can do better than rely on the skill of trained inspectors to prevent explosion, by giving timely warning of dangerous weaknesses or defects in the boiler or its attachments.

Mr. Le Van: My object in preparing this paper was based on the very points which the gentleman has named. The explosion at Conshohocken cost about $100,000 to pay for all the losses. With all their trained inspectors, and all the facts before them, the Hartford Insurance Company insured the remaining boilers. The fact of a piece of iron showing well under a test is not a proof that the boiler is good. It is the boiler that wears out—the iron—the rivets, etc.; and no living man can tell when that point

commences. At the Cocheco Print Works in Dover, New Hampshire, I am informed, they remove their boilers every ten years, and they say that it pays; and had Messrs. J. Wood & Bros. replaced their boilers every ten years, I think they would have saved themselves a great loss. There is no doubt, from the evidence at the trial, that there was only fifty pounds steam pressure at the time of the explosion. I say the only safe way is to limit the time of use. I am sure that any establishment that can afford to pay for the losses resulting from explosion can afford to buy new boilers.

Mr. F. B. Allen: I cannot conceive, Mr. President, how any arbitrary rule can be laid down, since the strength and security of boilers depend on so many practical conditions. To condemn a boiler after eight, nine, or ten years of service might be very safe; but it is not only the old boilers that explode, many new boilers explode too.

Mr. Pierce: I am not a member of the Society, but I should like to ask a question of those gentlemen who are experts in boiler inspection. Some time since I made very careful tests of specimens taken from the plates of a boiler after explosion. I found that one of the plates was $\frac{5}{8}''$, $\frac{3}{8}''$ scant, and $\frac{1}{64}''$ in thickness, and the thinner plate was originally $\frac{5}{16}''$, and certainly at the time of measurement was not scant $\frac{1}{64}''$. I found that the thinner plate would stand a tension of an average of 48,000 and some hundredths pounds, whereas the thicker plate only stood a tensile strain of about 28,000 pounds. The thick plate was brittle, whereas the thin plate was found to be in excellent condition. The boilers were there over twelve years, certainly. Now the question in my mind was whether the boiler was not as likely to explode when first put in as it was at the time of the explosion. The punching and workmanship in the manufacture of the boiler were gross. The boiler was so grossly put together that it seems to me it would be utterly impossible to arrive at any decision with regard to the question of time, and that the present system of inspection is the only safeguard we have, except an improvement in the system, if an improvement can be made.

Mr. Hutton: It has been my custom, in discussing the subject of the wear and tear of boilers in service, to classify the wear and tear into two great divisions: The wear and tear which are preventable and the wear and tear which are inseparable from the way in which boilers are customarily hung, and from the neces-

sary variations in pressure to which those boilers are exposed when heated during the day and cooled at night; and it seems to me that from the wear and tear due to the unavoidable use of the boiler—the necessary flexing of the shell when at night it becomes oval, and its return to the cylindrical form when pressure is increased during the day, there must necessarily be a bending strain at all the joints. Of course those familiar with inspection know the phenomena which result no doubt from that very effect occurring regularly from day to day. We have simply to assume that there is something corrosive in the water, either from natural causes or the presence of decomposed fatty acids. We can only tell by inspection whether that has gone to the point of danger. Then, of course, there is the longitudinal flexure of the shell. When it is cool at night it assumes a certain position. When heat is applied the tendency of the boiler is either to hook up, or the reverse when the bending strain comes the other way. Then, of course, there is the bulging action on the head. These are entirely unpreventable from the necessities of the generation of steam; and it is possible, perhaps, to put a limit at which it would be unsafe to use a boiler, from the simple matter of age. But there comes in over and above all these unpreventable things in the use of the boiler, all the carelessness—usually careless stoking—which assists these processes, which perhaps in the normal use of the boiler would wear it out in twenty years, and the boiler may be unsafe after it has been in use one year. Of course, inspection can only guard us against that, and I think that ought to be discussed in regard to the life of a boiler.

Mr. J. B. Root: I think Mr. Le Van's suggestions as to limiting the time for which shell boilers may be used a wise one; but there is another way of avoiding all this danger, and that is by encouraging the use of sectional boilers. A barrel of gunpowder, if ignited, will do immense damage; but if the same quantity of gunpowder is made up into fire-crackers it cannot hurt anybody. The same rule should hold good with boilers, and I think it is the only way in which steam will ever be harnessed so as to avoid danger.

Mr. Le Van: I would like to ask Mr. Root if any pattern of boiler has ever been built that has not exploded?

Mr. J. B. Root: Many have been built, which, if they did explode, could not do any harm. Take a boiler made up of a number of small compartments. There is not enough in any one of

those compartments to cause great destruction. If one of those compartments is ruptured, it causes a gradual lowering of the pressure. As to scalding, accidents of that sort may, of course, take place with the sectional boiler, but those accidents that deal destruction all around are what we want to prevent. That is the true principle; adopt the sectional form of boiler. It can be applied to all the uses for which the steam-boiler is used, provided the proper amount of attention is given to it. We hear this matter of boiler explosions discussed all the while, and still we seldom hear anything said in favor of the sectional system. It is the only cure for steam-boiler explosions and their disastrous consequences. It would be a great deal better if more encouragement was given to certain forms of boilers that cannot explode and cannot produce disastrous effects.

Mr. Le Van: Boilers made up of a number of small compartments, to be successful, have to have large steam domes, partially filled with water, thereby reducing them to the same condition as those of the ordinary boilers in general use, as regards safety.

Mr. J. B. Root: You take a drum filled with steam only, and there is very little expansion in it. In case you get a hundred pounds of steam on it an expansion five times will reduce all its pressure. I have seen some air-chambers where they were testing them for compressing air, and they compressed the air up to several atmospheres, and the men stand alongside the vessel. Rupture it, and it might flop over a little, but it would do no harm; but if filled with water heat it up to a hundred pound pressure and let the vessel rupture—what is the consequence? A large part of that will fly into steam and increase in volume seventeen hundred times, and that is what causes destruction. Mere steam-drums are not very destructive, even if they do rupture.

Mr. F. B. Allen: The matter of design enters largely into the safety and economy of boilers, and while we may make the vessel in which the steam is generated very safe by making it of a sectional form, there seem to be some practical disadvantages connected with that system, so much so that the introduction of such boilers as the gentleman described is retarded. They have been successful in a great many places, but the difficulty of keeping them tight, the varying expansion, the expense attending the renewal of the parts, and other things, operate to retard their more general introduction. The question has narrowed itself

down to getting a boiler whose design shall be such as to be as safe as possible, and to embody the requisites of a good boiler in the necessary fire surface and disengaging surface for the steam.

MR. J. B. ROOT: I do not see that it is the imperfection of the sectional system that has retarded its general introduction so much as other considerations. We have a large rolling mill interest, and insurance interest, and large vested interests all over the country, which are of course conservative, and do not want change, and it takes time for a change of that kind to come into use. But I believe if the thing is looked at rightly, it will be seen that the results produced by the sectional form of boiler have been as good as those produced by any other form. So far as economy, durability, and cost are concerned, they are just as good. The only thing against them is the vested interest and vested prejudices. We are after safety in steam-boilers, and that is the only means by which we can get it, and it will come in time when these obstructions have gradually worn away.

MR. COTTER: I wish Mr. Le Van had been a little more explicit as to the lifetime of a boiler. I am not a boilermaker, nor an insurance man. I will take a practical illustration of a locomotive boiler. I was called to look at one this past week which was made in 1868. It ran for six months and has not been running since. It has been taken the best care of. Now what shall we do with it? It has not been used for over ten years. When will the life of that boiler cease? I agree with Mr. Allen on other points. I do not think you can very well fix the life of a boiler arbitrarily. It depends a great deal on the care it receives and the water that is used in it, and a great many other things. As to the remedy being the use of sectional boilers, it is entirely impracticable to use sectional boilers in all conditions, and I think that in speaking of boilers we are speaking about cylindrical or shell boilers, and what we want is to get at some definite way of making them more secure. There are cases where a sectional boiler cannot possibly answer the purpose. I think Mr. Root can call to mind some conditions where a sectional boiler could not be made as suitable as other types of boilers. The English Board of Trade tried to establish a law, I think, some time ago, and I looked upon it as a very good one. Setting the tensile strength of a boiler at 50,000 pounds, and using it for ten hours a day for sixteen years, was, I think, the regulation. Something like that is what we want. But putting a boiler out of use, because it has

not been in use for a certain number of years, without regard to the character, or the continuity of that use, does not appear to be reasonable. I know of boilers that have been running twenty-five years, and I cannot condemn them. I do not know what to say more than that they are in use that length of time. The best we can do is to inspect boilers as well as we can under the present system, until somebody will propose to us some other and better way.

MR. WATSON: It seems to me, sir, that the only remedy for this matter, which the members have touched upon, is thorough care, examination, and inspection of all the parts. I can call to mind numberless instances where boilers have been used severely quite up to their limit, and are just as good, practically, as on the day they were first put in. Such a pair of boilers are now on the steamer "Old Dominion," of the Old Dominion Steamship Line. They have been in use ten years. They have been in charge of a practical engineer, a capable man, and there is not a particle of scale anywhere about them. To all intents and purposes, and to all appearance, they are just as good as they were the day they were put into the ship, and I fully believe, from what I have seen of them, that they are as good. The mischief would arise here: If you instruct the working engineer that his boiler is liable to fail at any time, say within a limited period, ten years or so, from crystallization of the iron, you put him in constant fear of the boiler, and you tell him that it is of no use to take care of his boiler; that it is liable to fail at any time through its inherent defects. It is detrimental to the interests of steam-engineering, in general, that a man should be told that his boiler is liable to fail from such causes at a certain time. The only remedy, I think, is thorough care.

MR. LE VAN: My object in writing this paper was with reference to the ordinary use and inspection of boilers. If all boilers were well made and properly inspected I admit that there would be no reason for removing them at the end of ten years. I examined some boilers a few weeks ago in Philadelphia at the instance of the owner, and I wrote him a letter telling him that they should not run another moment. He had them insured. They were only one-sixteenth of an inch thick in places, and when I saw them they had 90 lbs. pressure on them. I drove my hammer through the iron. They were only twelve years old. If we do not have a law to limit the time, we must have a law to

make them inspect properly. The inspection law is a farce, from the fact that it is not carried out as it should be.

Mr. Cotter: I would like to ask the gentleman what was the type of the boiler, and through what portion he drove his hammer.

Mr. Le Van: Flue boilers. I drove my hammer through the mud-drum. When I was a young man I had the honor of being an apprentice under Mr. Horatio Allen, who is now present, and he sent me on one occasion down to Staten Island to inspect a pair of boilers. I found them carrying 90 lbs., and a few days afterwards I also drove a hammer through them, by a slight blow.

Mr. Cotter: The reason I asked the question was, that if we went to tearing down the brickwork every time we looked at a boiler, it would be troublesome.

Mr. Le Van: How can we inspect them properly without doing so?

Mr. Cotter: I do not think the brickwork interferes often with proper inspection. That is the reason I asked you where it gave out.

Mr. Le Van: Brickwork does interfere. The defect was, as I stated, in the mud-drum. I do not see how one can look through a brick. That has been my experience, and that is why I want a law passed to make them inspect properly or limit the time of use.

Mr. Lyne: I do not believe it is a safe principle to say that a boiler ought to be renewed once in seven years, or ten years, or sixteen years. I believe it ought to be renewed when it gives evidence that it is dangerous. I claim that a boiler should be so set that all parts would be accessible for inspection and repairs. A few weeks ago I saw a boiler taken out in the lower part of this city, which was said to have been in use twenty-four years, and so far as I could see, the boiler put in was far inferior to the one taken out. I have seen a set of tubes completely destroyed in two months, and one of those tubes is in existence at the present time. There was a process of pitting which began at the time, and within two months it pitted those tubes so that they were completely destroyed. I think that the proper way to determine the life of a boiler is to inspect it often and thoroughly. I just had occasion to investigate a case in Jersey City, where there is no law to require even a safety-valve to be put on a boiler. I think

the great difficulty and the real source of evil is in not having proper legislation, requiring the examination of boilers. If you ask a manufacturer to pull down his brickwork so as to enable you to examine the boiler, he will start you out of the place very quick. I think we need legislation requiring rigid inspection within certain limits, to see whether the boilers are in proper order and safe, or not.

LV.

RAILROAD ECONOMICS, OR NOTES AND OBSERVATIONS FROM THE OHIO STATE RAILWAY INSPECTION SERVICE.

BY S. W. ROBINSON, C. E., PROF. MECH. ENG. STATE UNIVERSITY, COLUMBUS, OHIO, MEMBER OF THE BOARD OF INSPECTORS UNDER THE HON. H. SABINE, COMMISSIONER OF RAILROADS AND TELEGRAPHS.

A LAW of the State of Ohio makes it the duty of the Commissioner of Railroads to inquire into the safety of the means employed in railway travel. To this end an inspection service has been organized, which aims to examine and report the condition of all the railroads of the State. While engaged in this duty the following items of information have been gained:

TRUNK AND OTHER LINES.

In Ohio, as well as in at least two other States, there appear to be two classes of railroads,—first, the great trunk lines connecting the West with the East, and, second, those having largely or altogether local interests. The former are most likely to run east and westward, while the latter run mostly north and southward. Strong companies control the former, while the latter often fail to pay well enough to keep up repairs. In many instances a weak company sells its interests to a stronger, when the former evinces a general need of repairs. The strong company or trunk line then puts the road into good running condition, sometimes to form part of a through line, sometimes a branch, and sometimes to form a tributary to it. In this way a road of secondary importance may be kept up to good running condition, while otherwise it would go down.

Whatever may be said against consolidation of railroads, it appears to be a fact that roads owned by strong and wealthy companies are in far better condition than otherwise. Indeed the generally good condition of the great trunk lines and of their branches is a credit to those companies. If these roads are backward in some things, such as introducing the best systems of "signalling," "blocking," and of "interlocking apparatus," they are certainly up in other matters, such as steel rails, iron bridges, etc.

SAFETY.

A little attention to railroading will suffice to show that safety in railroad travel is secured at the price of incessant vigilance. That a stretch of three hundred miles, extending across a State, is, every foot of it, perfectly safe to-day, is not proof positive that it will be so to-morrow, though the broken rail or washed culvert is the subject of constant search.

PROTECTION OF RAILROAD STRUCTURES.

The life of a railroad plant is not great. New roads, with iron rails and wooden structures, will need renewals for the most part within ten years. Rails endure according to traffic, and for light traffic will run ten years. Ties will rot out in from five to eight years. Culverts, cattleguards, etc., about the same. Good wooden bridges, when new, will be dangerous in ten years unless covered. If covered at all it should be done within two years after building, otherwise the timber becomes affected with dry rot at the heart. This decay might perhaps better be called *blind rot*, because it is hidden. A wooden bridge, nicely covered and painted, may *appear* to be in the best of conditions, but really be in the very worst. Joints in the lower chord of such bridges are seen to be pulling out by the locks splitting off. In such cases, when the timbers are sounded with a boring-bit the latter will find sound wood for two or three inches, when suddenly the bit may take a jump of four or six inches through a dry-rot hole. Such well-covered and well-appearing bridges are found not to have been covered under about three years after building. Equally good uncovered bridges, even better, ten years old, have been found as those of equal age, well covered, in which the covering was delayed three years. It appears that after three

years of exposure to open weather, a bridge is doomed to a life of only about ten years, covered or uncovered.

But by prompt covering of wooden bridges the life is more than doubled, from which it appears that the practice of covering such bridges is highly economical.

It is sometimes the practice to cover simply the trusses, and it is necessary in "half Howe" or "pony" trusses of wood. This leaves the floor system exposed, and any sap-wood about the floor-beams or the stringers is soon eaten away with decay. Sap is of but little worth after three years' exposure, even when free. But heart-wood is often perfectly sound at ten or fifteen years. Sap-wood is so comparatively worthless that some engineers specify that not over eight per cent. of section of timbers shall be sap. It is an excellent precaution to thus limit the sap-wood, because it is practically of no value. In existing bridges sap-wood rot has reduced the section of chords, as estimated, from ten to twenty per cent., the remainder being sound. Uncovered flooring should, therefore, be watched, and when the beams are found weak, as by observed excessive deflection, new beams should be added.

Painting is an excellent practice, and its power for prolonging the life of wood is not confined to free or external surfaces, but internal as well,—that is, to illustrate, lower chords have been examined where the wooden "clamps and keys" were laid in white lead, or sometimes in red lead, and such are sound and strong to a greater age than unpainted.

A close joint in wood, where exposed, is far worse than an open joint of small space sufficient for air to pass. From this fact it appears that wood contacts have been avoided by using iron "clamps and keys" in lower chords. Some engineers make iron clamps or blocks with a space for ventilating between wood and iron, the bearings being quite narrow. These have given good results, and point to the value of ventilation.

As to ventilation in general, all coverings should leave the main bridge timbers free for air to circulate about them. For instance, the boarding along the side of trusses should be firred out by girt strips being nailed to the truss, along the braces above the lower chord and below the upper chord, and not on the chords themselves. Then, when the boarding is nailed upon these girts, it stands out free, so that air can freely go all about the chords.

In some instances chords have been found covered with tin, the

same being fitted about the braces and nailed to the chords, so as to appear like giving protection to the chords beneath. But this is believed to be worse than no covering whatever, from the simple facts that, first, water will work in at the numerous joints, and, second, be held there by the tin covering. If the tin could be carried away from the wood by a 2″ space, the latter being allowed for ventilation, it will serve a good purpose when it is made tight. These conditions are readily met in "combination" bridges, that is, in such as have wood upper chord braces and end posts, but with iron ties and lower chords. The upper chords are readily covered with tin, because nothing protrudes above to prevent. The braces, or vertical pieces generally, do not need covering, as it is found that the wet so rapidly escapes as to leave the braces soon dry.

Special pains should be taken to keep wet out of close places in wood. For instance, in deck bridges (Howe's), water is apt to leak through the roof, as it is difficult to lay a roof among the ties, floor-beams, string, etc., in the floor system and get it tight. In such cases the sway braces are apt to carry the water which falls upon them down upon the lower chord. This has been avoided very neatly, cheaply, and efficiently on the Lake Shore & Michigan Southern Railway, by making a saw cut across the top and edges of the sway braces, and driving in a collar of sheet iron or tin, which extends down like spurs below, and thus heading off any water which may find its way through the floor or roof above, and alight upon the sway brace to come trickling down upon the lower chord.

But though tin may be suitable to cover upper chords as above explained with reasonable durability, yet as a main roof covering over the tops of Howe bridges it appears to be utterly worthless, for the reason that the sulphurous fumes of the smoke from the locomotive soon eat the tin roof through like a big pepper-box lid. Indeed this action upon iron has been observed upon heavier masses of iron than tin; the truss rods even having been observed in badly rusted or pitted condition, with a weakening of probably five to ten per cent. The latter has been observed to be most serious in low lands, such as would be frequented by fogs. The moisture of the latter deposits upon the rods and absorbs the acids of the smoke. The iron is then etched more or less seriously. Rods for such localities should be made with some excess of section to provide for the corrosion.

WOODEN BRIDGES.

The prevailing wooden bridge is the Howe truss. It consists, as generally put up, of an upper and lower chord, connected by vertical tie-rods running through with nuts at both ends; the latter dividing the span into panels containing braces and counter-braces. The chords usually are made of four sticks, side by side, with blocks or "keys" notched in, but leaving a space between all the sticks. Chord bolts run through from side to side of chord to draw all together. In short chords the sticks run from end to end. But for lengths greater than about forty feet, pieces are put in so as to break joints. In upper chords these simply abut against each other, but in lower chords, clamps are used to make tension splices. These clamps are generally of oak wood, and preferred by some builders of the first and by some of the second form in Fig. 156. Sometimes only one is used to a

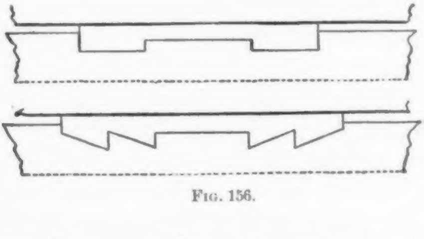


FIG. 156.

splice, as shown, but more often two, one on each side of pieces joined. A chord bolt goes through near each end of a clamp. The earliest point of failure in a wooden bridge is at the locks of these clamps, either on the clamp itself or on the interlocking hooks of the chord.

Two of these splices are never found opposite in a chord, but break joints, so as to allow one joint to each panel. In this way the so-called keys help to form the splice. There are always two main braces and one counter-brace between in each panel. The former always incline toward the middle span point. In moderate spans there are always two tie rods at each inter-panel point, but in long spans there may be three at the ends of truss. The largest of these rods are 2″ in diameter; almost always threaded without enlargement of ends. These ties draw against straps on the outside of the chords, running from 1″ × 5″ down to ½″ × 3″ in section, and long enough to extend the width of the chord.

The braces almost always set square against iron angle blocks. The best of these blocks have flanges to prevent the braces from falling out of place. These blocks are often found broken, but the breakage is evidently due to carelessness in drawing up the tie rods too tight upon the braces, because on some roads these breaks are very numerous, while on others the same make of blocks are never found broken. Road-masters say they find difficulty in getting men to draw their ties up properly upon the braces.

The depths of these trusses vary. Short spans, or "pony" trusses, sometimes also called half trusses, run from about eight to twelve feet height. Full trusses for longer spans are usually about twenty feet. The smaller trusses have about three or four floor beams per panel, while the larger have five. They always rest upon the chords.

When wooden bridges show signs of failure the speed of trains is often reduced till a remedy is applied, either in strengthening the bridge or by renewing it. Two ways of strengthening a lame bridge are in use. One consists of springing a wooden arch from the abutments, usually from iron skewbacks, placed about six feet down, and rising at mid-span nearly to the tops of the trusses. Each case requires four arched ribs, one on each side of each truss. From the arches the trusses are suspended by rods. But this method is too expensive for an old bridge; it is more common for a light bridge otherwise good. The second way to doctor the bridge, and which is very common for old bridges, is to put a trestle bent under at the third or quarter span. A pile bent is sometimes used instead of a trestle bent. The objection to placing the bent at the middle is the fact that the counter-braces near the middle in that case become main-braces. Some less considerate road-masters place the doctor at the middle, however. But as the carrying power of a truss varies inversely as the square of the span, other things being equal, it appears that the strength of a bridge is nearly doubled by placing a support at the one-quarter point. Such trestle bents are carefully watched to guard against washing out by the stream. In high water such a trestle bent is a treacherous affair, a pile bent being far preferable.

The lateral bracing in bridges is almost always about the same, viz., about $6'' \times 6''$ braces and $1\frac{1}{8}''$ tie rods, and the same from end to end of bridge. These are usually in the plane of the lower chords and also upper. No difference is made in the strength of

the lateral bracing, as far as observed, for straight or curved track, though it is certain that the centrifugal force of a train running on the curved track over a bridge will give cause for lateral thrusts, which are considerably greater than for straight track. One element of compensation, however, exists in the fact that bridges under curved track are usually wider, so as to allow equal clearance room, and this gives wider lateral trussing. Trusses are not found inclined on account of curves on the bridge.

Through bridges of wood have no "sway" bracing. The chords of the trusses are from 24″ to 30″ in breadth, and the floor-beams extend entirely over to the outsides. This keeps the lower chord in position. The braces cover about the whole width of the chords, so that the trusses are quite stable in erect position.

Deck bridges always have sway bracing, but in some cases much stronger than others. Where several spans of wood bridges are contiguous, in some cases both or all are made continuous from span to span. In other cases only one chord will be continuous. Diagrams taken, as hereafter explained, have shown that in continuous two-span bridges an appreciable rise of the second span occurs when the train gets fairly on the first.

IRON BRIDGES.

The prevailing form of iron bridge is the Pratt truss for long spans, and for short the plate girder. The change from one form to the other occurs usually at lengths between sixty and a hundred feet. These statements apply more definitely to recent practice than former. The older iron bridges are very promiscuous, both as regards form and manner of putting together. Some of the first iron bridges in the State were Howe trusses, one of which went down in the Ashtabula disaster. But in place of the latter we now find what is probably the strongest iron Pratt truss in the State, so that people need not now go around Ashtabula to avoid a second catastrophe.

The parts of truss bridges were formerly united in various ways, sometimes by bolts, notches, and locks, and often by riveting in place. But at present the method by pins and eyes prevails, especially for the longer trusses. In upper chords, however, though the tie rods are usually attached by pins, yet for increasing the rigidity they are made continuous by riveting on splice pieces extending past the pin-holes. The forms of parts of bridges, as well as the methods of joining, are almost as though

stereotyped. Thus the "eye-bar" is an article of manufacture, and is used in all parts of bridges except upper chords and struts or columns.

Upper chords and end posts are most frequently made of two channel bars, twelve to eighteen inches apart, with webs vertical. They are joined on top by a longitudinal plate extending the whole length and riveted to the flanges, while the bottom side is latticed or "laced." "Web" members serving as struts are most frequently composed of two channel bars at a distance apart, and connected by diagonal lattice slats riveted on. Sometimes, however, the two channel bars are riveted by their webs to the flanges of an I beam. Formerly Phœnix, Keystone, Box, and other columns, nearly or quite closed, were much in use, but they appear to have given place almost entirely to such open columns as above mentioned, simply from the necessity experience has developed of painting every inch of surface in iron bridges.

Most engineers require forms such that a paint brush can touch every part, either inside or out. The advantage of this is seen from the fact above mentioned of the rusting of truss rods and of tin roofs from moisture and smoke. The statement also, which has come to my notice, that tons of rust have been removed from tubular bridges, is in point.

Floor-beams, made by riveting four angle-bars upon the edge of a plate,—two upon each edge at opposite sides,—are the most common. The section approaches that of the I-beam. Plates are often riveted on top and bottom, part of the length, to increase the strength at the middle part where the moment of strain is greatest. These beams are most frequently suspended from the pins of the trusses by inverted "Ω" bolts. But when there is scant room below a bridge, for water-way or otherwise, they are in some cases riveted to the vertical struts.

The stringers are most frequently of rolled I-beams.

The lateral stiffening is much better attended to in iron bridges than in wooden ones. The lateral ties not only vary in size, from end to middle, but differ in size according to span, width, etc. There is generally a lateral system at both the bottom and top chords.

Plate girders are usually formed by riveting angle-bars to the sides of the webs, top and bottom, then across these a flange. The latter is increased by additional "lifts" laid on in the middle portion. These plate girders are usually of uniform depth, though

some have been met in which the upper flange or chord was arched so as to nearly join the lower chord at the ends. Vertical stays of angle-bars are riveted to the web throughout these girders, but nearest together at the ends, their object being to prevent the buckling of the webs. Most usually the two girders of a bridge are joined by riveting to the floor-beams, so that all forms a connected system. An angle-plate then is set between the floor-beams and girders, to prevent the latter from swaying or careening.

But on some roads many of the plate girders have wooden floor-beams; the latter sometimes resting directly upon the lower flanges and sometimes on angle-bars riveted to the girders. A neat and serviceable small bridge, where there is sufficient water-way, consists of plate girders, about ten feet apart, with lateral and sway bracings, and upon which are mounted the wooden floor-beams. This plan is carried down to I-beam girders of ten feet span or less.

In a few instances weak iron bridges have been strengthened by springing iron arched ribs from the abutments, composed of channel iron, and securing the same to the trusses at a suitable number of points.

At the present day many good iron bridges are found to be too weak. This is due not to any engineering defect, but to the growth in weight of freight loads and rolling stock. We now find sixty-ton locomotives where formerly there were forty; and twenty-ton loads per car where there were ten. Hence bridges designed to a strain of 10,000 pounds per square inch of iron section, as due to the former loads, must now stand 15,000, or perhaps more. This is unfortunate, since an iron bridge is so difficult to strengthen in a satisfactory manner, and so difficult for the road-men to get renewed.

Expansion and contraction of iron bridges is provided for in supporting one end on rollers. In short spans, however, rollers are dispensed with, and the bearing plates slide. Often the observed expansion reported by bridge attendants does not account for the whole variation of length, even where rollers are in use. It is believed by some that the rolling resistance under so much weight is so great as to spring the piers where piers are tall. Thus it appears that strains, due to constrained expansion, may be too great upon chords to be ignored in calculating total strains.

STRAINS UNDER MAXIMUM LOADS.

In proportioning the parts of bridges for resisting their strains, a great variety of detail exists in the present practice. We find no "live" bridge engineer of to-day adopting a fixed maximum load per foot for all spans, even for the same road and the same trains; neither do we find the same fractional part of the ultimate or elastic resistance of the iron adopted for the allowable strain for all parts of any one bridge. The factor of safety is "a thing that was" to such an engineer.

In the first place the quality of the iron is allowed to differ for different parts of bridges. Tension members are never made of anything but "double refined" iron, that is, iron that has been double rolled. This consists of taking "muck-bars" (the result of first rolling from puddle blooms), cutting and piling them, reheating to a welding heat, rolling into bars, then cutting, piling, and reheating again, when they are rolled to the needed sizes. Compression pieces are single refined, in which the last piling and rolling above described is omitted. Channel bars of columns and upper chords are thus treated.

A fair quality of double refined iron in bars should have a tensile strength of 50,000 pounds per square inch; an elastic limit of 26,000 to 30,000 pounds per square inch; should stretch fifteen per cent. in eight inches; bend 180° around a cylinder of diameter equal its thickness without fracture; and when nicked and broken should show a fibrous structure. Such iron in the regular truss tension members is usually allowed to be strained to 10,000 pounds per square inch for the maximum load. In some cases floor-beams are allowed 8000 pounds only, because they are strained nearly to the maximum allowed for each passage of load. This is true of floor-beams, because the greatest load occurs when under the drivers of the locomotive. In the main truss, however, the maximum strains are only reached when the whole train is up to the maximum, a condition which does not happen with every train. The Ω-shaped hangers for floor-beams are usually allowed only 5000 to 7000 pounds. Struts and upper chords are computed as columns, and on a supposed basis of about 8000* pounds per square inch. This low value

* By a rational formula for columns, published by the writer since this paper was presented to the Society, it is shown that this value should be but a little over 6000 pounds. See Van Nostrand's Eng. Mag., for June, 1882.

is probably partly due to the fact of single rolling for channel iron.

ASSUMED MAXIMUM ROLLING LOAD.

In calculating the maximum strains there are two ways of treating the question of the maximum load.

1st. By adopting the greatest actual train weights, such as two of the heaviest locomotives, followed by a train of the heaviest loaded freight cars; then computing the strains as static effects, to which results are added, for "dynamic effect,"

For spans of about 30 feet,	25 per cent.
" " " 50 "	15 " "
" " " 75 "	10 " "
" " " 100 and over,	0 " "

2d. By assuming fictitious train weights which are uniform per foot for the span, but which are much the greatest for short spans. Thus, for some roads on this plan, the assumed load for calculating strains is,

For spans of 10 feet,	6000 lbs. per foot.
" " 40 "	4000 " " "
" " 150 "	3000 " " "
" " 500 "	2500 " " "

This diminishing scale is to be accounted for as providing, first, for impact; the latter being greatest for short spans, because so much more quickly passed by the forward end of a train, and causing an application of load which is so sudden as to be of the nature of a blow; and, second, because short spans have the locomotive itself for the maximum load, while longer spans can only be covered by adding to the one or two locomotives, some portion of the train.

CRYSTALLIZATION OF IRON IN BRIDGES.

As regards the deterioration of iron in use by crystallizing, there are differences of opinion and too few facts. One man will present evidence of crystallization, while another will produce equally good evidence against it. It appears that data are too uncertain. When rods taken from a bridge are found to be crystalline, it is not known whether they were not so when put in. But this matter will be settled in due time, because positive data now exist as to the condition of iron in existing bridges.

When the future engineer shall examine the parts of these bridges, and compare notes with the former records, we shall know how about crystallization.

STEEL BRIDGES.

We are now at the verge of a steel bridge era, several important steel bridges being already built, and in process of construction. The most important mechanical difficulty in this direction is already overcome in the existence of machinery for the manufacture of solid steel eye-bars. Steel is in every way better fitted for bridges than iron. It is less subject to deterioration, becoming more uniform in results of manufacture, has an ultimate strength of nearly double that of iron, and an elastic limit from two to three times as high. Considering the strength, it is but little, if any, more costly. This step from iron to steel is but the natural course from cast-iron up, which latter material is now entirely abandoned as a material for bridges, except for unimportant members, such as wall plates, packing pieces, etc.

SWAY BRACING.

In both wood and iron bridges "sway" bracing is universally employed in deck bridges. But such bracing is held in doubt by some, except at the ends of the bridge, where it should be especially strong. The reason given for this belief is that where one truss receives a greater strain than the other from any such cause as wind against the train, train at one side, as in double track bridges, curved track, etc., each truss should be allowed to remain in a plane. But the sway braces preserve the cross-section, so that if one truss deflects more than the other, each truss must careen to one side to some certain corresponding extent at the mid-span, but not at the ends, because here the solid abutments prevent. This forces the chords laterally out of a straight line, causing horizontal transverse strains upon them. The eye-bars on one side of the lower chord would, under these circumstances, be strained more than those on the other side, an inequality which would disappear in the absence of sway bracing. Not only would the main trusses be affected, but the lateral bracing at top and bottom would be strained unduly, and probably higher than provided for in the oversight of this matter. The old Ashtabula bridge was an iron deck, and who can say to what extent the sway bracing was responsible in the failure of it?

Though the one consideration of greater flexibility of cross-section seems to favor the omission of sway braces, in that we thus obtain freedom from stresses in one system of bracing as due those in another system, yet it is probable that the yielding cross-section will allow the train, while under wind pressure, to be forced to a greater inclination toward the leeward, thus causing a probable greater displacement of the centre of gravity of train toward the leeward truss, and increasing the strain on the latter.

VIBRATIONS AND STRAINS.

In observing the deportment of a bridge as a swift train passes over, the parts are seen to be much agitated. Tie rods will often fly about at the middle parts to a very considerable extent. This has evidently received some attention by engineers, because in a few instances tie rods at the crossing points have been found tied together apparently to stop vibrations. That all such vibratory movements cause direct strains in the vibrating parts there can be no doubt; and it is unfortunate that these vibrations cannot be predetermined so that the strains resulting from them can be calculated. Could these be accurately determined, it is probable that the practical maximum working stress for bridge iron in tension could be safely raised from 10,000 pounds per square inch to 15,000 pounds; a margin being still left between the latter figure and that for the elastic limit for indeterminate strains due to such movements as considered below.

LURCHING OF THE BRIDGE.

In some cases the whole central part of the bridge is also in an agitated condition, both vertically and horizontally. There seem to be various causes for this, such as want of perfect balance in the drive wheels and connections, error in perfect alignment of rails, especially in the vertical plane, wandering of the wheels from side to side over the 1″ to 1½″ of clearance between flanges and rails, irregularity of curves on bridges, tangent points on bridges, etc. In some cases this seems to amount to an oscillatory or vibratory movement of the whole bridge.

A BRIDGE INDICATOR.

In order to study these effects more satisfactorily, as well as the "dynamic effect" of a moving train upon a bridge, an instrument has been devised which might be called a *bridge indicator*, the

object of which is to give a graphic record of the movements of a bridge as a train passes it. A rude affair of the kind has been used with results given below. (See Fig. 162.) In this case a bridge near Columbus, Ohio, was chosen as the subject of experiment with the instrument, it being the only one yet experimented with. This particular bridge was a "pony," or "half Howe" truss, of two spans, both upper and lower chords being continuous over the central pier. Each span is 60′ 6″ long, with a total depth of truss of 8′ 9″. The chords are of three timbers, 5″, 10″ and 5″ × 12″ in section for the lower, and 5″, 10″ and 5″ × 9″ for the upper chord. Main braces are 6″ × 8″, and counters 6″ × 6″.

The upper diagrams of Fig. 162, numbered 1, 2, 3, etc., were all taken at the middle of the west span of the bridge. The lower diagrams, of the same numbers, were taken at the middle of the west half of the west span. Thus, any two diagrams under one number were taken simultaneously, the upper at the middle and the lower at the quarter of span.

The track on this bridge was straight, except at the west end, where ten feet belong to a curve of about four degrees. Thus, a tangent point lies in about ten feet from the west end. The object of placing an indicator at the west quarter of the bridge was to observe the effect of this tangent point.

A description of the instrument will aid us to a better interpretation of the diagrams. At each point for taking diagrams a wooden board, dressed smooth, was secured to the bridge firmly at one truss. The plane of the board was vertical and perpendicular to the line of the truss. A paper was secured to the board by thumb tacks for each diagram. Upon these sheets while thus tacked to the boards the diagrams were made. At the midspan the paper faced toward the east, while at the west quarter it faced toward the west. From the ground beneath the bridge a stand was built of timbers and brought up to where a pencil could be firmly held by it, and in such position as to lightly touch the paper tacked upon the board secured to the bridge as above described. Under these conditions a movement due to the yielding of the bridge in any manner would be indicated by a mark of the pencil upon the paper. A vertical deflection of the bridge would make a vertical mark equal in length to the deflection. Also a horizontal movement would be indicated by a horizontal mark, or, finally, any sort of cross-motion of the bridge at the indicator would be evinced by its representative mark. In other words,

the bridge autographically registers all of its own transverse movements.

The same figures would be obtained, evidently, if the paper were held upon the stand and the pencil upon the bridge, except one would be inverted with respect to the other. The most natural arrangement is the latter, and for that reason the diagrams in Fig. 162 are so posed that a downward motion of the bridge is indicated by a downward stroke of the pencil on the figure. The figures of the plate are enlarged 2.7 times.

No. 1 was taken at the middle of the bridge when a slowly moving freight train was passing, drawn by an ordinary sized locomotive. The pencil was held on the paper till about ten cars had passed going east. The bridge sank gradually from A to C as the engine approached the middle of the span. But as it passed on over, the pencil rose to D, and remained there till about five of the heaviest loaded cars passed. For the lighter cars following, the pencil rose to E and remained there for the next five cars, and it was then removed.

No. 2 is for a freight train going west at about twenty miles per hour. A is the position of the pencil when the bridge is at rest. As the engine came upon the east span the pencil rose from A to the top of the figure, and then descended again to the bottom as the engine came over to the middle of the west span where the indicator was located. Then the pencil rose to the top of the open part of the figure when it was removed, the engine having just left the bridge. The lower part of No. 2, taken at the quarter, had the pencil in contact longer than the upper part; the heavy blotch at the top of the lower third occurring while the cars of the train were passing.

No. 3 resulted from the passage of a passenger train of four cars going west. As the train struck the east span the pencil rose from A to B, but descended as the engine came upon the west span to the lowest point, it then rose to the heavy markings at the middle. Finally the pencil returned to A.

No. 4 is for a passenger train going west. As the engine came upon the east span the pencil left the point of rest A, rose to B while the engine was on the east span, went to the lower part of the figure as the engine came on the west span, but finally returned exactly to A as the train left the bridge.

No. 5 is for a passenger train going east, four cars. Pencil went down to lower point as the engine was on the west span,

then it rose as the second span was reached, and finally went above A to B as the rear of the train was on the east span. But the pencil finally returned to A as the train left the bridge.

No. 6, passenger train, two cars, going east at about thirty miles per hour. Pencil was removed just as the last car passed it. This explains the absence of the point B. A variety of small movements must have occurred when the pencil was about at the middle of the diagram, thus giving cause for the black blotch.

No. 7 was taken as a pony-engine passed very rapidly going east. The pencil was removed as the engine reached the middle of the span. This explains why B is missing. The lower part of No. 7 is a mere simple diagram than any of those taken at the quarter, though the pencil was not removed till the engine passed. This is due to the fact that the engine was alone. This card gives us a complete loop, the pencil returning to A.

The diagrams from the quarter point add but little interest. They resemble the others both as regards general form and in having two points, A and B. They are smaller than the others, but not so much so as would be naturally supposed. They do not add much light respecting the influence of the tangent point on the west quarter of the bridge. Also the relation of the movements of the bridge at the two points does not appear to be systematic in detail, though bearing a general resemblance as above stated.

Much interest attaching to these diagrams is obscured in the knotted points. To remedy this it is proposed to arrange a clockwork to carry the paper forward, at a predetermined speed, while the diagram is making. Then if the number of cars in the train is noted, and if the instant at which each end of the train passes the indicator is marked by a dot on the moving diagram paper, we will, by knowing the speed of the paper, have data for miles per hour of train, and ordinates for every position of train. But on the paper we should have two curves traced, one for the vertical movements of the bridge, and one for the horizontal. This would give us the means of completely analyzing the obscure parts of the diagrams of Fig. 162.

Simple lurches would be indicated by irregular sinuosities without law, while for vibrations they would be systematic.

One drawback to the general applicability of this instrument would be found in the inconvenience in erecting the tower for

carrying the pencils. As a substitute for the tower, it is proposed to throw out a stone anchor from the desired point of application to the bridge, the anchor having attached a hempen cord or fine wire long enough to extend up to the point of observation. A pencil is then to be arranged in a slide working freely in vertical guides, to which slide the wire is to be attached. A spring, quite flexible, is then to draw upon the slide, making the wire below tense. Then as the bridge rises or falls the wire causes the slide to remain at a constant height, while the instrument and paper are vibrating with the bridge. It is then only necessary to place the pencil to the paper, and the clockwork in motion, to secure the diagram for the vertical movements.

The lateral movements are not quite so easily provided for, since there is need of an anchorage at one side on a level. It is believed, however, that this can be secured in effect by two anchors and chords, the latter forming a junction at the horizontally opposite point desired. To hold them, a tension strand under spring action, drawing as a resultant force to the two anchor chords, will fix the junction point as desired. In case of such double anchorage to the lateral and vertical, two pencils may be made to write on the one sheet or ribbon, and thus one clockwork answer the purposes fully.

Such an instrument with conveniences for anchorage could be applied to a bridge in a few minutes, and inspectors could obtain an autographic record of the degree of agitation of any and all bridges examined.

Such diagrams would evidently throw much light upon the vibratory effects due to unbalanced locomotive drivers, and indicate whether cumulative impulses from such parts of the train ever cause dangerous vibrations of whole structures.

INDICATED DYNAMIC EFFECT.

The diagrams presented on Fig. 162 are not sufficient in themselves to serve this purpose fully and satisfactorily. Their appearance might, however, suggest some amount of vibration or oscillation. Referring to No. 7, first part, remembering that the pencil was removed as soon as the engine reached the midspan, we observe some evidence of lateral vibration as occurring simultaneously with the sinking of the bridge. But as to the vertical movements, we see almost no trace of repetition of any part of the movement as would be likely to occur if the bridge vibrated

in going down, except, perhaps, in a slight degree in the loop in the bottom. This loop is about one-eighth of the depth of the diagram. The lower part of No. 7 indicates almost no vertical vibration in any part. Loops at the bottom of Nos. 6, 5, and 4, indicate vertical vibration, also of about 22, 14, and 10 per cent. of the depth of the diagrams respectively. Taking a half of these amplitudes as the increase of deflection due to dynamic effect, and comparing with the diagrams diminished by the same, we obtain the percentage which the dynamic is of the static effect, as 7, 12, 8, and 6 per cent. respectively, as due to the above measurements. Some of the lower diagrams give evidence of about the same percentages. The mean of these percentages is only about half what is required by some railway companies to be allowed for spans of the same length, viz., sixty feet. As given above in speaking of the usual practice in this matter, it is about 15 per cent. for sixty feet spans. But it is always necessary to provide not for average, but maximum stresses in such cases. Hence the maximum 12 is close enough upon the 15 of practice.

Nos. 1 and 2 are both from freight trains, and give evidence of almost no vertical vibration. Also the total deflections, counting from the points of rest A, are less for the freight trains than the passenger trains in Nos. 3 to 6. If, however, we add the above twelve percentage of dynamic effect to the deflection in Nos. 1 and 2, we obtain very nearly the same strains as are actually due to passenger trains, and singularly enough, as obtained in actual practice by computing static effect of freight trains and adding the stated percentage for dynamic effect.

These facts, though corroborative of the real existence of dynamic action or impact, yet at the same time they testify to a somewhat excessive allowance for it by practical engineers. But before drawing conclusions in this way for guiding us in practice, it is necessary that much more extensive data be procured and worked up.

As regards lateral vibration, the first two numbers on the plate are narrower than the rest, the same being taken from passing freight trains. The others are for passenger trains, except the last one, from a rapidly moving pony-engine. Hence it appears that fast trains cause much the greatest lateral disturbance. The resulting effect upon the lateral bracing is a matter of interest. By measurement of the widths of the diagrams taken at the mid-

span, it is found that the total lateral movement for passenger trains is 42 per cent. in excess of the like movement for freight trains. May not this call for careful attention to the subject of dynamic effect upon lateral bracing?

TESTING AND SELECTING MATERIAL.

In the selection of material for bridges, great care is exercised by bridge companies, much greater, indeed, than is usually supposed by the mass of people who ride over their bridges. Some bridge companies make tests of the materials not specified or required by the railway companies ordering. For instance, the Detroit Bridge Company examines all the eyebars for a bridge by piling a quantity of them and passing the pin through the eyes at opposite ends simultaneously. Any bar preventing the passage of the pin is thrown out. Then the bars are individually tested to a tensile strain of 15,000 pounds per square inch, and again the pins must similarly pass. If any eyebar has stretched so as to prevent the passage of the pin it is rejected. Such a practice would discover hidden flaws, and would pay if discovering such flaws only at the rate of one in a hundred bridges. A flaw which would probably have been made known by such a test was actually discovered by the roadmaster of the Baltimore & Ohio Railway in one of his iron bridges, and the piece had to be removed. A first-class catastrophe might have here resulted except for the keen eye of the roadmaster. There are those who object to straining iron going into a structure, especially beyond the working load. But a test which will discover the few hidden flaws that would otherwise pass unobserved, will probably more than offset imaginary evils due to strains which, though within safe limits, are somewhat in excess of the adopted working load. Accordingly this test is believed to be a most excellent one, but of the few bridge companies conferred with in regard to it by the writer, it has been found in use only in the one instance named.

All companies do more or less testing with testing-machines, including pieces ranging from small "test specimens" to full-sized bridge members. Tests for tensile resistance are by far more plentiful than compressive, but a good number of the latter are on record, including full-sized bridge columns. It is a quite common practice, however, to test a piece taken from a large bar rather than the whole bar itself. Large bars are thus found to have a lower tensile strength than smaller rolled bars.

Testing-machine tests for tension, to meet the present demands of bridge builders and companies, must make known at least three quantities:

1st, The elastic limit.

2d, The ultimate strength.

3d, The percentage of total elongation of some specified portion of the original bar,—usually about eight inches.

In some cases the greatest reduction of section is noted, and by some this item is preferred to the percentage as above.

As regards the elastic limit, it is found not to be perfect, that is to say, some permanent elongation is always experienced by good iron before arriving at what is usually adopted for that limit. But practically these elongations are nearly proportional to the increments of load, and extend nearly through the whole range of loading up to the so-called elastic limit. Beyond this limit, however, they rapidly increase. The point where this change takes place is noted as the elastic limit. This limit, thus found, is given a more rational showing from the fact that, if at any point within it, the strain be relieved and then restored, no further permanent elongation is experienced till after passing the previous condition of strain. At points beyond the elastic limit, however, this is not the case. An extended examination of iron specimens will verify the following facts:

1st. Bars immediately from the rolls, which have not been subjected to jars or other causes of strain, will experience permanent elongation at very slight tension. This is true also of bars direct from the annealing oven, even though they had previously been subjected to violent mechanical action. In these cases there appears to be no limit of perfect elasticity.

2d. A gradually applied and removed tension within the usually accepted elastic limit produces a permanent elongation, which will not be increased for like or less tensions as above stated. This is also true of compression.

3d. A specimen which has been strained, as indicated in 2d, will take a permanent set for a slight reversal of the strain.

4th. At the point where the permanent elongations cease to be nearly proportional to the increments of load, or to the elastic elongations, we find the usually accepted elastic limit. Some of these facts can be verified by simply straining a piece of annealed wire by hand.

But the most-common tests in use among bridge builders, and

which are at once both invaluable and fortunately of easy application by any blacksmith, consists,

1st, of bending a bar 180° around a cylinder whose diameter equals the thickness of the bar, and which the bar must stand without fracture to be accepted;

2d, of nicking a bar on one side with a cold chisel, and bending it similarly as in 1st with the nick at the bow of the bend, when it will usually break, showing a fracture which must be fibrous and free from glistening points or faces. Very frequent use is made of these tests in the smiths' workshops, where waste pieces of bar ends, which have no other value except for scrap, are put to a most valuable service.

HEADS OF EYE-BARS.

In the manufacture of one very important part of iron bridges, viz., the eye-bars, a number of methods are in use. One consists of forming the heads by a separate operation and then welding them upon the bars. The weld is made close to the head without upsetting the bar near the welding-point. This must certainly reduce the sectional area of the bar at points so near the weld as to be heated but not worked, because the heat cannot be taken without corroding the iron, and thus eating away a small portion. But where full-sized bars of this kind have been tested to destruction, it appears that the rupturing-point is always at some intermediate part of the bar considerably removed from the head, thus proving the reduction by burning to be unprejudicial. This practical ignoring of the slightly reduced end sections appears to be due to the influence of the enlargement of the head. This conclusion is verified by experiments in tension on extended necks of wrought iron, the fracture always occurring at some intermediate point in the neck.

In other cases heads are formed on the bars by welding several thicknesses of iron upon the side of the bar, thus giving a sufficient body of metal to form the eye. The reduction of section above mentioned, by fire corrosion, will take place here also, but actual experiment has shown it to be without objection, for reasons above given.

STONE ARCHES.

Of stone bridges there are some fine ones in the State, particularly on the Lake Shore & Michigan Southern Railway, the Bal-

timore & Ohio, and the Cincinnati, Cleveland, Columbus & Indianapolis. The former has four or five large stone arch bridges, two or three of which are two span, and they run from 40 to 80 feet diameter. Also one beautiful two-span skew arch of about 20 feet diameter. At Bellaire the Baltimore & Ohio Railway has a remarkably fine stone viaduct, consisting of thirty-seven semicircular arches, of 28 feet diameter, supported on piers 6 × 12 feet. The height of the copings above the streets of Bellaire is 32 feet. Twenty of these arches are in a straight line, and seventeen on a four-degree curve, all in dressed stone.

STONE QUARRIES.

In the selection of stone from Ohio quarries for important structures, care is needed lest a soft stone be taken which will not stand the weather.

THE ROADWAY.

Ordinary railroad lines consist of four parts, viz., *bed*, *ballast*, *ties*, and *rails*. A cross-section of the most perfect roadway found in Ohio is given in Fig. 157. The Pittsburgh, Fort Wayne & Chi-

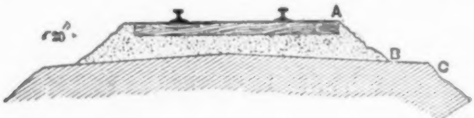


FIG. 157.

cago Railway has some seventy miles of it, so fine in its outlines as to be truly a work of art. It is, indeed, unfortunate that all passengers cannot conveniently see it from the moving car. Observed from the rear car of a train, it appears like a beautiful striped ribbon stretching away in the distance. Across the top the stone ballast is just to the upper surface of the ties. At A a definite line of intersection is formed. At B is another and also at C. The slope A B is as perfect as though the ballast had been piled under a board. The limit of ballast at B is by a single row of ballast stone, between egg and nut size, and individually laid by hand. The upper surface of the bed is crowned or convex, as shown in the figure. The part B C is all patted smooth with shovels. A weed is not allowed. The ballast is broken stone where this form of bed is found. The road, however, has not all stone ballast, though the amount is increasing from year to year.

Other roads have considerable portions laid with stone ballast,

that ballast being much sought after. Cinder or slag from furnaces is also employed, it being preferred to some kinds of stone.

Some Ohio stone is entirely unfit for stone ballast, and does not pay for hauling it from positions of convenient proximity. It pulverizes in use. Limestone is said to be the preferable stone. In considering what material shall be declared the best ballast, it appears that a best ideal ballast must be heavy enough to not be easily disturbed when laid, and to hold the ties in ballast; it should not be too fine nor too coarse, say about egg size; it should have sharp angular corners to hold the ties, and it should be impervious to water so as to dry out quickly for preservation of ties. Probably the best possible material for uniting all these conditions is broken glass. It weighs about the same as limestone. Glassy furnace slag comes very near to it. Sandstone is the poorest of all stone, since it wears rapidly so as not to hold the ties, and it absorbs moisture and holds it, to the rapid decay of the ties. But impervious stone allows rain water to run directly through the ballast to the bed by trickling down the surface of the fragments and without absorption. On reaching the bed it flows off to the right and left if the bed is sufficiently crowned at its summit. In the best practice it is actually crowned for this purpose.

The minimum depth of ballast shown in Fig. 157, is six to eight inches under the ties. It often actually exceeds this, sometimes to the depth of several feet. Two reasons are given for this: first, a new bed settles, causing inequalities of grade; and second, inequalities of grade admitted in new roads are, to a considerable extent, equalized according to the growing importance of the road. In these cases, rather than add new bed material to revive settling annoyances, ballast is piled on.

TIES.

Ties used in the State are mostly oak; the best being obtained from Virginia and known as "Virginia ties." They are of white oak and run from 10″ to 12″ width. Chemically treated ties of elm and some other woods have been used to some extent. The number per mile varies between 2500 and 2800.

SLIP SIDES.

In a few instances "slip sides" have been encountered, in which the whole fill or embankment, for a length of one or two

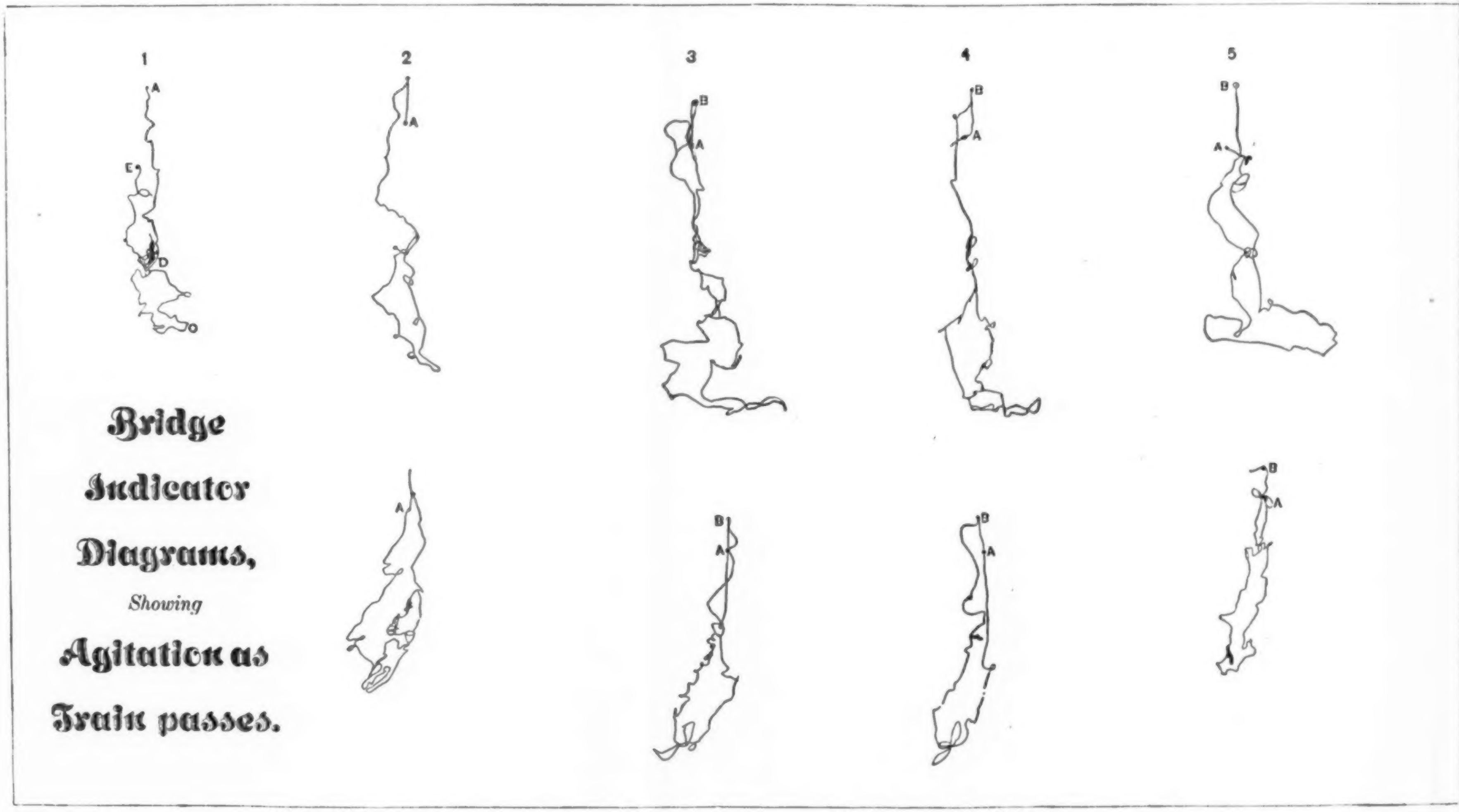

1
2
3
4
5
A
B
E
D
O
Bridge
Indicator
Diagrams,
Showing
Agitation as
Train passes.

Fig. 162.

neers.

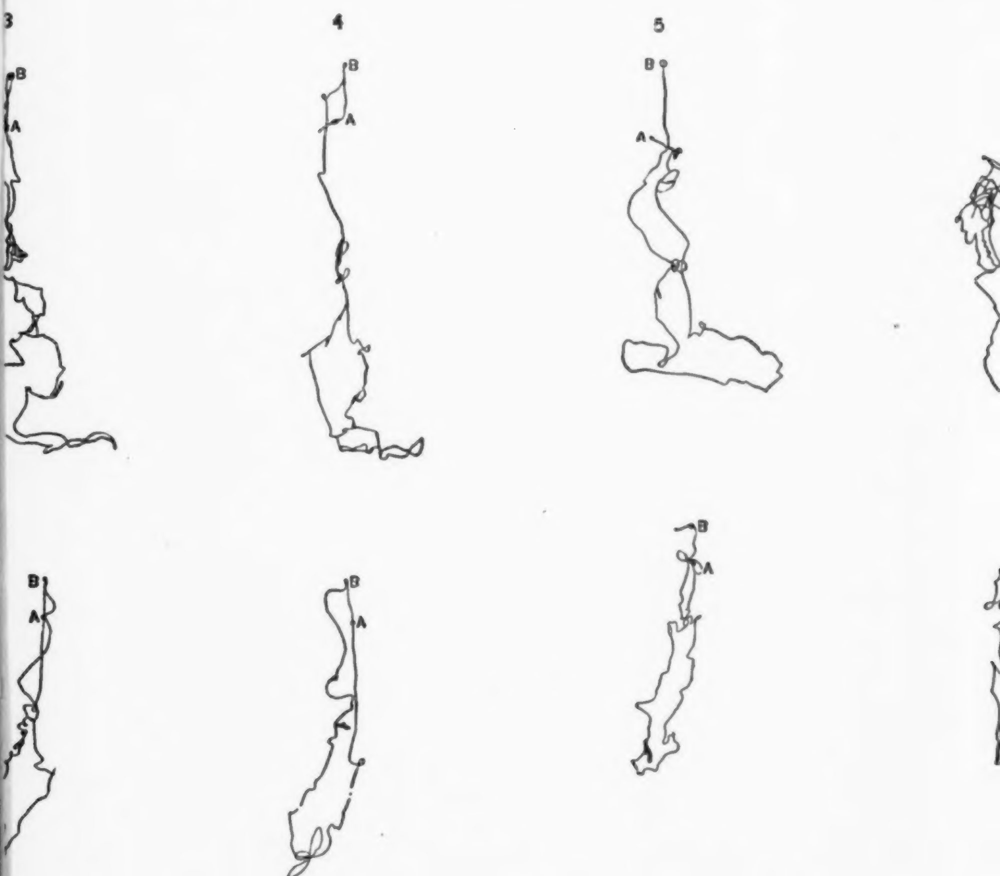
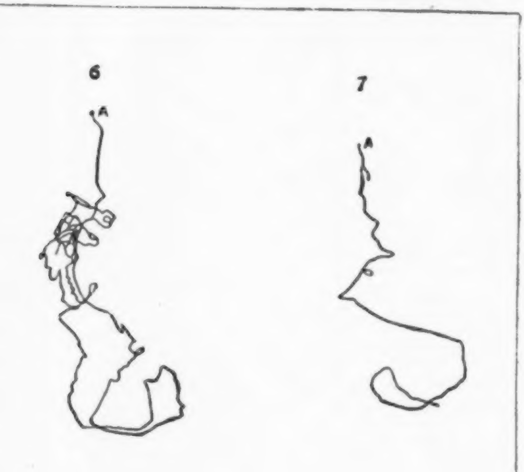


Fig. 162.

ROBINSON.



hundred feet, will gradually be carried laterally out of place. These are found very difficult to manage. In one case, on the Baltimore & Ohio Railway, a filling of coal slack was used after several additions of earth filling had been carried away. The coal stood very well, its less specific gravity being supposed to be the cause. In some cases piles are driven, the idea being to "pin" the slipping bank to place. But these pins often get badly demoralized from the great pressure. Springs of water are usually found along the upper limits of these slipping sides.

THE TRACK LINE.

The alignment of the track on curves often gets deranged to a surprising extent; in one case over forty per cent. by measurement was the degree of curvature raised. One instance, on the New York, Pennsylvania & Ohio Railway, was noted where a curve carried the train badly. Several unsuccessful attempts were made to correct it by throwing the track by eye. Finally the curve was re-run with instruments and found badly out. In many cases the track has been observed to be appreciably deranged where measurements were not taken.

Such derangement occurs by the working of section men on the road, as in readjusting grade, or outer rail elevation; in placing new ties, rails, etc. Tangent points undoubtedly "creep" from this cause, the presence of them a feet in upon bridges, as noticed in a few instances, being apparently due to it.

Track men should have some easy and simple means of ascertaining the deformity of curves and the proper "elevation of the outer rail." For the latter an extraordinarily simple and efficient device was found in force on several roads, viz.: a cord or tape line of certain length, say 60 feet, and a rod; the former being stretched as a chord to the curve, the versed-sine measured on the rod, is the elevation of outer rail. Some use 63 feet, and others less for the chord length. For the 63 feet the elevation is right for about a 36 mile speed.

In this device we find the suggestion for a curve corrector, viz.: at all points of an ordinary circle curve the versed-sine, for the 63 feet chord, should be of constant value.

In regard to the tangency of straight and curved portions of track, the usual practice is to make the curves true circle arcs, and exactly tangent to the straight parts. A little consideration, however, will show that instead of this, the path described by the

centre of gravity of a car should preferably have its corresponding parts thus in true tangency. But this cannot be where the outer rail is elevated or inner one depressed, or both, because in tilting the car for this difference of rail elevation the centre of gravity is thrown in, and passes around the curve on a circle arc several inches within the circle which is truly tangent to the straight parts of the path. This has the effect to give a jolt to the car on entering upon a curve. But, in practice, this is compensated in a measure by commencing the elevation of rail on the tangent itself at some distance from the tangent point, and bringing it up to the full value at or near the tangent point.

The object of making a difference of rail elevation on curves is to make the resultant of gravity and of centrifugal force take a position which shall be normal to the floor of the car. To secure this result perfectly, in every respect, it is evident that we can neither begin the elevation on the tangent nor admit of anything less than full value on the initial part of the circular curve. Neither should there be any offset, sudden or gradual, in the path described by the centre of gravity of the car, such as above mentioned as due to rail elevation. Abrupt disturbances in the direction of this resultant would be perceived as jolts toward one side or the other. It is evident that the direction of a disturbance which would be least noticeable to a passenger, or have the least tendency to derail a train, would be vertical, and hence this is the most admissible. But it appears impossible to preserve quietude in every respect in a car, even though the resultant force above named could be maintained truly in the normal position indicated, because the car must be rotated on some longitudinal axis to the extent of the difference of rail elevation. This necessitates an elevation of one side of the car, depression of the other, or a compromise action, the latter being probably preferable. Hence one rail must be depressed as well as the other elevated, the best condition being obtained when the centre of gravity of the car is neither raised nor lowered.

Under these conditions, viz., first, maintenance of perfectly normal resultant, and, second, a slight rotative movement of the car on its longitudinal axis, we secure the least possible disturbance. Then the only sensation to a passenger, if indeed any be possible in going round a curve at the proper speed, would be that of slight lifting or lowering, as depending on sitting at the lifted or lowered side of the car.

But it is clearly not possible to realize these conditions when a straight track is, according to custom, changed abruptly to a circle. Not even though the circular curve and tangent belong to the centre of gravity of the car, instead of the middle line of track. The only way to fulfil the conditions indicated, appears to be to gradually increase the curvature from the tangent to the circle by an intermediate curve of varying curvature. This we will term an *easement** curve; the main circular curve beyond the easement curve being called the principal curve.

Now the easement curve must, throughout its length, maintain perfect continuity of proper relation of the radius of curvature and rail elevation. That is to say, to meet the above conditions, the radius of the easement curve must change from point to point; and the rail elevation at any one point must be precisely that required for the radius at that point. This relation of elevation and radius is well known, viz.: the elevation is simply in the inverse ratio of the radius. Or, again, the product of the elevation and radius of curvature is a constant for any given number of miles per hour for speed of train. This is true whatever the form of the easement curve, and hence the latter is neither determined nor influenced by that relation.

Being free to assume the law of the easement curve, it appears that the very best conditions possible to adopt for fixing it are to assume, first, that the car, in tilting to the difference of rail elevation as it passes along the easement curve, shall rotate about a longitudinal axis passing through the centre of gravity of its cross-section; and second, that it be accelerated in that tilting movement, so that a passenger at the side of the car shall experience only the sensation of a slight change in his own weight while on the easement curve. That change of weight will be an increase if outside and going from the tangent, and *vice versa*. This change of weight, however, should be made imperceptible, and it is believed so to be when arranged as below.

This makes the law relating to the time and rail elevation identical with that of falling bodies, or with

$$h = \tfrac{1}{2} f t^2$$

where h is the elevation, f the constant acceleration, and t the

* Called curves of "*easing changes of curvature*," and "*curves of adjustment*," by Rankine, also "*spiral curves*," by others. See *Rankine's Civ. Eng.*, p. 651; *Railroad Gazette*, Dec. 3d, 1880; recent articles in *The Engineering News*, etc.

time. Now suppose that in running this easement curve, 50 feet chords are adopted $= c$.

Let the number of chords reckoned from the tangent point $= n$. Also assume that at 400 feet from the tangent point the elevation of one rail over the other be 12.8 inches. Let the number of chords passed per second by a passing train be $t, = n\ c$. Substituting these values, and reducing for a velocity of 30 miles per hour, we obtain

$$h = 0.2\ n^2$$

From this we obtain for

$n =$	1	2	3	4	5	6	7	8
$h =$	.1″	.8″	1.8″	3.2″	5.0″	7.2″	9.8″	12.8″
$R =$	17190	4297	1910	1074	688	478	361	286

n being the number of 50 feet chord lengths from the tangent point, R the radius of a curvature, and h the difference of rail elevation in inches, for a track gauge of 4′ 8½″.

For a speed of 40 miles per hour, for the same radii, R:

$n =$	1	2	3	4	5	6	7	8
$h =$	.36	1.4	3.2	5.7	9.0	12.8	17.4	22.8

It is observed that the radii are the same for all cases. This makes the easement curve the same curve for all speeds, the allowance for different speeds being made in the elevation. Hence the curve can be laid out from the same set of deflection angles, computed once for all.

In a particular case of practice, the easement curve is to be continued to where its radius equals that of the *principal*, or main circular curve; when the latter is to be run tangent to it in continuation.

Now these curves should be understood as forming the proper path for the centre of gravity of the car, and not the centre line of the track. For greater convenience to passengers, however, it should be the path to the centre of gravity of the load of passengers. But as these centres do not differ much in position, they may be assumed coincident.

Assuming this centre of gravity to be at a height above the track equal to the gauge of track, viz., 4′ 8½″ usually, it appears that in order to make the path of that centre of gravity describe the easement and principal curves above laid down, it will be necessary that the curves, when first laid out on the ground, must be moved outward at each point a distance which just

equals the difference in rail elevation, h, at that point. This is to provide against displacing the centre of gravity as the car tilts to the difference of rail elevation.

Hence, in practice, run the easement curve, as above, till its curvature equals that of the principal curve. Then set out each point the amount h proper to it as rail elevation. Then continue on the principal curve. In laying the track depress the inner rail the same amount that the outer one is elevated, both together being h. This is to be done for that speed of trains at which it is desirable to have the most perfect freedom from all manner of disturbances.

In compound curves not reversed, the easement curve should be introduced to give a gradual change of curvature, rail elevation, etc., from one curve to the other. In reversed compound curves, the easement curve and elevations should be used to change from the first principal curve to where the track would run off on a straight tangent, and then it is to be run, in the inverse order, to where its curvature equals that of the second principal curve, etc. In short, every portion of principal circular curve should begin and end in an easement curve, as described above.

This gives perfect freedom from side jolts and a probably imperceptible vertical lift or decadence. To give an idea of the latter effect, that is to say, of the apparent gain or loss of weight, suppose a man of 200 pounds weight to be at the extreme side of a car, and that the car enters upon the above easement curve at 30 miles per hour. The accelerative lifting or depressing force due to the 200 pounds weight will be, by calculation, only 0.16 of a pound, or about $2\frac{1}{2}$ ounces, an effect which would influence the cushion of the car-seat less than to place an orange in the rider's lap.

But all the above refinements respecting the alignment in the horizontal plane will be of but little avail where the importance of the vertical alignment is ignored. From an extended examination of track, both by sightings from the ground, and by taking advantage of opportunities of riding miles within one or two hundred feet of a second track, and of allowing the two lines of rail to spin through a fixed gaze with a view to observing the relative heights of the two rail lines, it is believed that the error of vertical alignment is usually at least five-fold greater than in the horizontal.

Of two sections of road, if one should be found as badly out in

the horizontal alignment as the other in the vertical, each otherwise correct, it is altogether probable that the section-boss of the former would get his discharge the first time the road-master came along, while the other would very likely be commended. But in this case the wrong man is discharged, because, as to the riding qualities of the two sections of track, the former would be far the best. This fact is evident by observing that the weight of the car is sure to cause it to follow all inequalities in the vertical alignment, while most of the lateral deviations of the rail will be skipped, and pass without effect. But even if followed to detail, in both instances, the vertical deviations will rock and tilt the car badly, and cause disturbances which will be magnified by the height of passengers or freight above the track. To explain, suppose one rail perfect in line, and the other to rise and fall one inch, in distances of fifty or a hundred feet. Here one wheel has a latitude of vertical movement of one inch. The straight rail forms an axis to this motion, and if a circular cylinder, of radius equal to the track gauge, be drawn to this axis, cutting the car lengthwise, every point in that cylinder would have the one inch of motion. That is, a point vertically over the straight rail, at the height of track gauge, would move sidewise one inch when the wheel on the opposite rail rises and falls one inch. Persons in seats directly over the straight rail receive the lateral jolts of about one inch. But persons in the seats at the opposite side of the car receive jolts which are both vertical and lateral to the extent of about one inch, which amounts to a diagonal jolt of about one and a half inches. The top of the car may, at the same time, be thrown two or three inches.

This supposes one rail straight, but it is as likely to be out as the other; both sometimes together and sometimes opposite. In case both rise together one inch, the car receives the vertical displacement bodily of one inch.

But when they are in discord, the passengers are thrown to an extent nearly double that due to the single rail error above. The consequent jolting annoyance cannot safely be prevented by rigid car-couplings, because the strains would be great upon the couplings, and no coupling attempts it.

But now suppose equal inequalities in the lateral direction or in the horizontal alignment. The wheels would skip most of them, the tendency being to go nearly straight ahead rather than turn out for all side-crooks in the rails. This is rendered possible

by the clearance between the wheel-flanges and rails. The cars are prevented, to a great extent, from wandering from side to side of the clearance by use of couplings, which offer a considerable resistance to the lateral movement of one car end, crosswise to the one coupled to it.

From these facts it appears that the vertical alignment is the one which demands the most careful attention for exactitude, while in practice it seems to receive the least.

"Low joints" are found everywhere, though in the most carefully guarded track they are slight. Where the "fish-plates" are allowed to get loosened, the wheel pressure and peening action bend the rails to an arched form. Small "joint-ties" also favor low joints.

If the rail-joint could be given the same stiffness as the body of the rail, and then if the bearing of the ties upon the ballast could be uniform along the rail line, the rails would remain straight. The "angle-bar" is superior to the fish-plate for making a stiff joint; but as no joint in use is as strong as the body of the rail, it follows that the deficiency should be made up by a greater amount of tie bearing near the joint. There are many advantages in the so-called "suspension joint." It is formed by placing the abutting ends of the rails over a space of about ten or twelve inches between two ties, so that the fish-plate or angle-bar will span the space, and be secured upon the ties. The advantage of this in the matter of low joints consists in the greater amount of tie bearing upon the ballast at the locality of the joint, and due to the fact that these two ties are nearer each other than other ties along the rail. But still the tendency is to low joints, and it seems necessary that, in laying ties, the two widest ones be selected for the pair at the suspension joints. This, together with closely-fitting angle-bars, it is believed will maintain freedom from low joints. This is based upon the supposition that the ties along the middle portion of the rail be all smaller than the joint-ties.

But in actual practice the joints in one line of rails are sometimes placed opposite those in the other line, and sometimes the joints alternate. Some road-masters strenuously insist on opposite joints, others equally so on alternate, and each will have no other. This is the one thing about railroads on which there is found the greatest prevailing difference of fixed opinion.

Now, respecting the bearing of the joint-ties upon the ballast:

first, when the joints are opposite, we find that the selected wide ties, which become joint-ties for one rail line, are also in proper position to serve as joint-ties for the other line. This also leaves the middle portions of the rails resting on the smaller ties, a condition pointed out above, as favorable for preventing low joints. But when the joints are alternate, the wide ties selected must be twice as numerous, and consequently differing less from the remaining ones, but besides this we find that the wide ties for the joint at one side extend across, and become wide ties at the middle of the opposite rails. This favors low joints, as pointed out above, and is one reason why alternate joints should be avoided.

But some contend that alternate joints ride more easily and pleasantly than opposite. This is probably true for equal degrees of low joints, but it seems to be an open question whether the alternate joints, with their greater tendency to low joints, will carry trains more smoothly than will opposite joints well laid on the selected joint-ties. But on some roads very little if any attention is paid to selecting joint-ties. In such case it is probable that alternate joints will ride smoothest.

It might be supposed that alternating low joints would give an oscillating motion of car from side to side, and opposite joints a vertical oscillation. The latter is likely to occur for speeds under about twenty-five miles per hour. But in high speeds the time between joints is too small for serving as a period of vibration or oscillation. Hence it cannot take place. At thirty miles per hour, alternate low joints appear to be entirely without effect for all oscillation, and is not noticed at even twenty or perhaps fifteen miles per hour.

From these facts it appears that low joints can be more effectually avoided when opposite, but will have less prejudicial riding qualities when alternate.

RAIL SECTIONS AND WEIGHTS.

The form of rail section is a matter of considerable import. The prevailing modern form is nearly like No. 1, Fig. 158, while some of the older rails in use in the State are nearly like No. 2. Various devices have been used for making the joint in No. 2, but it is a hard rail to hold. Fish-plates and bolts soon release their grip. The bolts are apt to break, but they first stretch and loosen the plates. Then the plates, rails, and bolts wear badly, because the form of section is seen not to be favorable for hold-

ing a fish-plate. On the other hand, the upper and lower parts appear much as though they would serve admirably as wedges to spread the fish-plates and tear the bolts. In some cases wood is used on one side, and sometimes both wood and iron.

But the nearly square shoulders between the head and foot in No. 1, are seen to be especially well adapted to hold a fish-plate. Even a little looseness under the fish-plate bolts would not admit

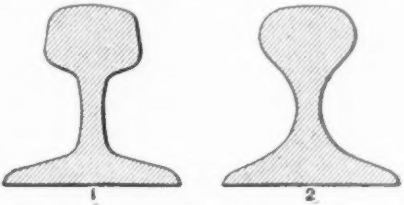


FIG. 158

of very much vertical displacement of one rail on the other at the abutting ends.

Rail weights vary on Ohio roads from 50 to 67 pounds per yard, the most common being 60. It is often the case that heavier rails are laid on curves than on tangents. This is to provide against the greater wear on curves.

LOCK-NUTS.

In spite of the fact of numerous existing lock-nuts of merit, none seem to meet all the requirements for fish-plate bolts. The simplest found in use is the Verona lock-nut. It consists of a split and offset ring of steel, tempered to a spring, and having cutting points at the split. It appears to be made of quarter-inch square steel, cut and bent nearly to a ring, but having an offset of about one-eighth inch, where the ends nearly meet to form the ring shape.

WEAR OF RAILS.

Practice develops the fact that the outside rail on curves becomes by far the most worn. In some cases the outside worn rails, and inside nearly perfect ones, are interchanged, so that each shall get its portion of wear. The wear now referred to is mostly on the side of the rail head. The tops of the heads also become much worn. Altogether the wear on curves is much in excess of that on tangents, a fact which accounts for laying heavier rails on curves.

The fine theory of the "coning of wheels" is entirely without force in practice. Wheeis wear most near the flanges, so that in a short time the effective coning is reversed; that is, the wheels become smaller in diameter of tread at points near the flange than at points remote from it. It seems evident that the more wheels become thus worn and lose their coning, the greater will be their tendency to climb outward on curves, and consequently the greater will be their slip and the greater the wear, not only of wheels, but also of the rails on curves.

The recent improvement in chilled car-wheels of leaving an inch at the rim-edge of tread, without chill, will doubtless tend to make the wear more uniform over the whole tread.

SWITCHES AND FROGS.

A great variety of notions about switches and frogs are found in vogue. For instance, some have decided preferences for the Lorenz switch, and others for the Wharton, where any other than the ordinary plain switch is desired. The Wharton switch is the homeliest switch, probably, that was ever made. It would never get adopted from any good looks. But it has great advantages for certain positions in track. A remarkable property of it consists in its leaving the main line of track entirely intact or unbroken. This is secured by means of parts so formed and raised as to lift the wheels high enough to carry the flanges over the rails at the one side. In practice, both sides are raised. This raises the cars also in passing the switch, a requirement which could not be admitted at high speeds of train.

Hence it appears that this switch is especially adapted to places where trains are to pass at high speed along the main line, but where it is necessary to occasionally turn out to a side track, the latter being always at a reduced speed. Being a "safety" switch, it is well adapted for yards and all places of much switching at slow speeds. While the Wharton switch requires one sharpened rail, the Lorenz requires two. These are so fitted that they will lie close up to a whole rail and receive a wheel from it. The Lorenz admits of two unbroken lines of rail, but one of them turns off to the branch track, so that one rail of the main line is cut. In this way it becomes unnecessary to raise the cars in switching. Hence this switch is adapted to locations where a train may continue on main line or take a branch at speed. This switch is made a safety switch by introducing a spring. But the

spring is seriously objected to by some with the statement that a stick or pebble may become engaged between the pointed and fixed rail, and thus throw a train. Devices have been introduced for obviating this objection.

Beside the ordinary "frog," two others are found in use, viz.: the spring frog, and the self-acting frog. Some roads are very partial to one or the other of the two latter, while others will have nothing to do with them. The chief objection seems to be that the movements are apt to become obstructed by sticks, dirt, cinder, snow, or freezing, etc. But on main lines, where turnouts are to be passed at speed, and on lines passing fifty to a hundred trains per day, a common frog is apt to become much worn in a comparatively short time. The spring and the self-acting frogs have far greater wearing qualities than the common frog, because they secure nearly the effect of an unbroken rail. Where switching is not frequent, and trains pass at speed, the Wharton switch and spring frog are good accompaniments.

BRIDGES ON NEW ROADS.

Economy in the management of railway structures favors the adoption of cheap wooden ones, such as trestles, pile bridges, etc., in the construction of roads, the same to be renewed in due time by more permanent ones. One important consideration in regard to this is the practical "water-way" under bridges. In some cases trestles a few hundred feet in length have been introduced at points of unknown water-way, which have subsequently been reduced to a complete fill, with the exception of a tile opening. In other cases iron structures have been undermined by reason of a cramped water-way.

The life of a wooden bridge is perhaps none too long for enabling the engineer to learn the actual demand upon any bridge location for the water-way. In one instance a fine Pratt truss of over one hundred feet span was placed over a nearly dry channel, at a height above bed of only about five feet. A stranger to the locality would wonder why the space was not filled with dirt, at a cost of almost nothing comparatively. But should he happen to be along at the one or two times a year when water was up, he would form an opinion, sound and correct, as to water-way.

CATTLE-GUARDS.

This structure, though of seeming insignificance, is yet of very great practical moment. This is due to two facts, viz.: first, the

great number of them required on a single road; and, second, that only one defective cattle-guard is sufficient to wreck a train.

The amount of attention given to this matter by different roads varies greatly. For instance, they have been found built, except the "strings," of ordinary rail ties, and without much designing either. Some roads have an almost infinite variety, most of them being built of such material and in such manner as is most convenient to the locality. Others will not only have a carefully designed and specified "standard cattle-guard," for universal use, but will have material lying in their material yards all along the

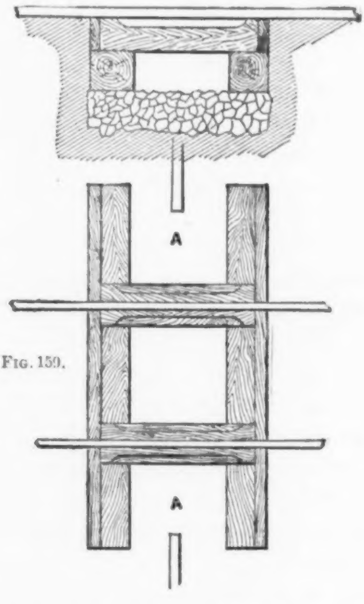


FIG. 159.

road, cut to specifications and ready for setting new guards, or for repair of old ones on the plan of interchangeable parts.

The latter system reduces the matter of cattle-guards to a basis of manufacture with all its advantages of economy, stock on hand, etc.

The three most characteristic standard cattle-guards, observed by the writer in the tours of inspection, are here described.

The standard of the Pittsburgh, Fort Wayne & Chicago Railway, shown in Fig. 159. A pit is first sunk nearly three feet deep, filled nearly one foot with broken stone. Then two square timbers, about 12″ × 12″ × 12′ are laid crosswise the track at each

side of the pit, and resting on the stone. On these, against the pit's banks, are laid, on edge, planks about 3″ × 12″ × 12′. Between these planks the "strings," about 12″ × 12″ × 6′ or 8′, are placed, one under each rail, as shown. The planks are heavily spiked to the strings, thus fixing the distance between the latter. The rails are spiked directly to these strings, the latter being chamfered. The fence, on either side, terminates about at A, as shown. An effort is always made to drain the pit by a channel, so that water scarcely ever lies about the pit timbers or mudsills.

In this guard the pit is generally left open, and is about two

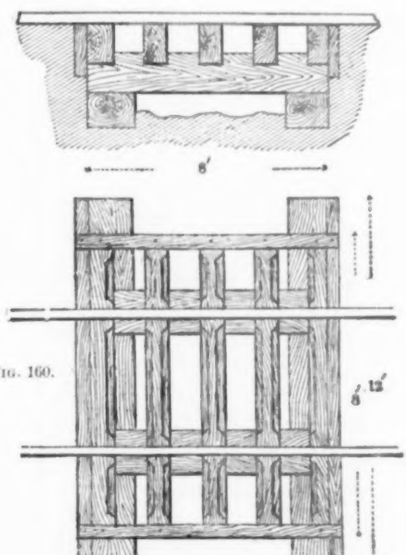


Fig. 160.

feet deep. Slats are sometimes put on, though this seems to be the exception.

The standard of the Cleveland, Tuscarawas Valley & Wheeling Railway is shown in Fig. 160. Two sticks, about 12″ × 12″ × 12′ are placed in the bottom of the pit, 8′, out to out, crossing the rail line. On these are placed two sticks or strings, about 12″ × 12″ × 8′, one under each rail. On these last are placed five sawed ties 6″ or 7″ or 8″ × 8′ and notched on to a remaining depth of 6″. The outer ties are placed flush with the ends of the sticks on which they are notched. Against the outsides of the outer ties, and extending down by, partly over the ends of the strings, are

placed planks about 3″ × 12″ × 12′ and spiked. Across the ends of the ties a binder or guard rail of wood is bolted. The rails are spiked upon these ties as on any other ties. The upper corners of the ties are chamfered.

This is believed to be a most efficient guard. Its cost, as compared with that of Fig. 159, depends mainly on whether the five

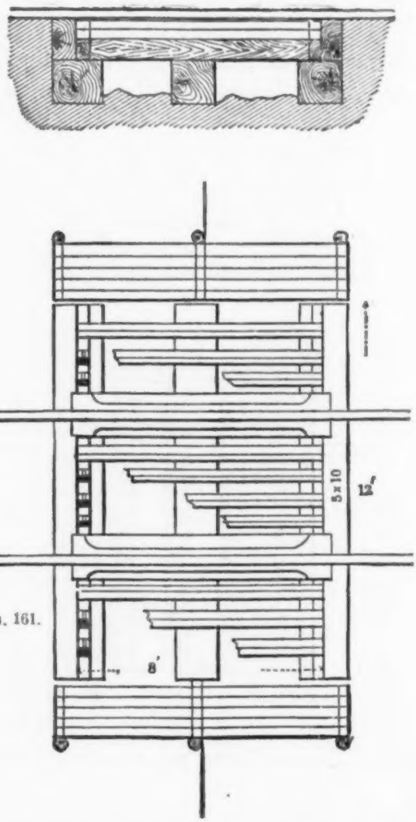


FIG. 161.

sawed and chamfered ties, and guard rails and bolts, of Fig. 160, cost more or less than the stone filling in the botton of Fig. 159.

The B. & O. Railway have a more elaborate and costly standard, shown in Fig. 161. In the bottom of a pit three mudsills are placed, each about 12″ × 12″ × 12′. Across and on these are laid the string pieces, one under each rail, and 12″ × 12″ × 8′. The ends of these are notched into 5″ × 12″ × 12′ planks, the outsides of the latter coming just flush with the two outside

mudsills. The strings are chamfered, and the rails are spiked to them.

Slats or bars are counted an essential part of their guard, and they are provided for in a most ingenious manner. On the inside of each of the outer heavy 5″ × 12″ planks is spiked a $1\frac{1}{2}$″ × 8″ × 12′ plank, notched at spaces of about 12″. The notches are diagonal, so as to carry 4″ × 4″ bars, lying in a diagonal position, that is, so that one corner of each stick is up. These bars are not nailed, because in a few months they sag, and are then turned over. As often as any one sags it is reversed.

At each side of the track, or end of the guard, is placed a half length of fence as shown, from which starts out the division fence to which the cattle-guard belongs.

Nothing would seem to be more efficient than this as a cattle-guard, and yet the road-master states that in one case a certain man's cow had educated herself to such a point of excellence that she would deliberately and safely walk the chamfered stringers, placing right feet one side and left feet the other side of the rail. But this isolated instance of successful climbing cannot be considered due to any fault of the cattle-guard.

GRADE.

The steepest grade noted by the writer is 85 feet to the mile, though that is very likely exceeded in the State. Very little attention appears to have been given to controlling grade, or least possible maximum grade, per division, or other portion of road. Neither to the matter of grade compensation for curvature. These questions appear to rise to great importance only on long stretches of road through uninhabited country like our western wastes, where it is not convenient to locate "helper engines" for an occasional excessive grade.

But in Ohio, a road-master will reply, stating the steepest grade, and give its location; and also that it is perhaps ten feet or fifteen feet steeper than any other. He may say, also, that in each case the grade was made as small as possible, regardless of reduction of cost of road by allowing the grade to go up to the controlling maximum at any point on a portion to which this controlling maximum belongs.

LVII.

A NEW METHOD OF KEEPING MECHANICAL DRAWINGS.

BY CHARLES T. PORTER, PHILADELPHIA, PA.

THE system of keeping drawings now in use at the works of the Southwark Foundry and Machine Company, in Philadelphia, has been found so satisfactory in its operation that it seems worthy of being communicated to the profession.

The method in common use, and which may be called the natural method, is to devote a separate drawer to the drawings of each machine, or of each group or class of machines. The fundamental idea of this system, and its only one, is keeping together all drawings relating to the same subject-matter.

Every draughtsman is acquainted with its practical working. It is necessary to make the drawing of a machine and of its separate parts on sheets of different sizes. The drawer in which all these are kept must be large enough to accommodate the largest sheets. The smaller ones cannot be located in the drawers, and as these find their way to one side or to the back, and several of the smallest lie side by side in one course, any arrangement of the sheets in the drawer is out of the question.

The operation of finding a drawing consists in turning the contents of the drawer all up until it is discovered. In this way the smaller sheets get out of sight or doubled up, and the larger ones are torn. No amount of care can prevent confusion.

Various plans have been adopted in different establishments intended to remedy this state of things, but it is believed that none has been hit upon so convenient, in all respects, as the one now to be presented.

The idea of keeping together drawings relating to the same machine, or of classifying them according to subjects in any way, is entirely abandoned, and in place of these is substituted the plan of keeping together all drawings that are made on sheets of the same size, without regard to the subject of them.

Nine sizes of sheets were settled upon as sufficient to meet our requirements, and on a sheet that will trim to one of these sizes, every drawing must be made. They are distinguished by the first nine letters of the alphabet. Size A is the antiquarian sheet

trimmed, and the smaller sizes will cut from this sheet, without waste, as follows:

A, 51″ × 30″; B, 37″ × 30″; C, 25″ × 30″; D, 17″ × 30″; E, $12\frac{1}{2}$″ × 30″; F, $8\frac{1}{2}$″ × 30″; G, 17″ × 15″; H, $8\frac{1}{2}$″ × 15″; I, 14″ × 24″.

The drawers for the different sizes are made one inch longer and wider than the sheets they are to contain, and are lettered as above. Those of the same size, after the first one, are distinguished by a numeral prefixed to the letter. The back part of each drawer is covered for a width of from six to ten inches, to prevent drawings, and especially tracings, from slipping over at the back.

The introduction of the blue-printing process has quite revolutionized the drawing office, so far at least as we are concerned. Our drawings are studies, left in pencil. When we can find nothing more to alter, tracings are made on cloth. These become our originals, and are kept in a fire-proof vault. This system is found admirably adapted to the plan of making a separate drawing for each piece. The whole combined drawing is not generally traced, but the separate pieces are picked out from it. All our working copies are blue prints.

Each drawer contains fifty tracings. They are two and a half inches deep, which is enough to hold several times as many, but this number is quite all that is convenient to keep together. We would recommend for these shallower drawers.

Each drawing is marked in stencil in the lower right-hand corner, and also with inverted plates in the upper left-hand corner, with the letter and number of the drawer, and its own number in the drawer, as for example, 3 *F*—31; so that whichever way the sheet is put in the drawer, this appears at the front right-hand corner. The drawings in each drawer are numbered separately, fifty being thus the highest number used.

For reference we depend on our indices. Each tracing, when completed, is entered under its letter in the numerical index, and is given the next consecutive number, and laid in its place.

From this index the title and the number are copied into other indices, under as many different headings as possible.

Thus all the drawings of any engine, or tool, or machine whatever, become assembled by their titles under the heading of such particular engine, or tool, or machine. So also the drawings of any particular part, of all sizes and styles, become assembled by

their titles under the name of such piece. However numerous the drawings, and however great the variety of their subjects, the location of any one is, by this means, found as readily as a word in a dictionary. The stencil-marks copy, of course, on the blue prints, and these, when not in use, are kept in the same manner as the tracings, except that only twenty-five are placed in one drawer.

We employ printed classified lists of the separate pieces constituting every steam-engine, the manufacture of which is the sole business of these works, and on these, against the name of every piece is given the drawer and the number of the drawing on which it is represented. The office copies of these lists afford an additional mode of reference, and a very convenient one, used in practice almost exclusively. The foreman sends for the prints by the stencil-marks, and these are thus got directly without reference to any index. They are charged in the same way, and reference to the numerical index gives the title of any missing print.

We find the different sizes to be used quite unequally. The method of making a separate tracing of each piece, which we carry to a great extent, causes the smaller sizes to multiply quite rapidly. We are marking our patterns with the stencil of the drawing of the same piece, and also gauges, templets, and jigs.

It is found best to permit the sheets to be put away by one person only, who also writes up the indices, which are kept in the fire-proof.

We were ourselves surprised at the saving of room which this system has effected. Probably less than one-fourth the space is occupied that the same drawings would require if classified according to subjects.

The system is completely elastic. Work of the most diverse character might be undertaken every day, and the drawings of each article, whether few or many, would find places ready to receive them.

Index photographed at the beginning for the convenience of the microfilm user.